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THE  
DOCTRINE  
OF  
PHLOGISTON  
ESTABLISHED,  
AND THAT OF  
THE COMPOSITION OF WATER  
REFUTED.

THE SECOND EDITION WITH ADDITIONS.

---

BY JOSEPH PRIESTLEY, L. L. D. F. R. S. &c.

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*Sed revocare gradum,——*

*Hic labor, hoc opus est.*

VIRGIL:

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THE  
DEDICATION.

TO SAMUEL GALTON, *Esq.*

DEAR SIR,

PERMIT me to endeavour to perpetuate, as far as I can the remembrance of your valuable friendship to me (as well as that of Mrs. Galton to my wife) which has continued without interruption from the time that we became acquainted on my settlement at Birmingham. The interviews that we have had at the *lunar society*, and on other occasions, I now look back upon with peculiar satisfaction, tho' mixed with regret. There is no lunar society to which I can communicate my observations, and from which I can receive light in return, in this place.

At my time of life, however, I could not expect to enjoy any society in this world much longer. Others, also, of our members, must now be looking forward, as I do, to a state of greater security and permanency than the present; where no riots will separate us again, and where we shall, I doubt not, resume our pleasing pursuits, and our speculations concerning the wonderful system of which we are a part, and with more advantage and satisfaction than ever. \*

There only can I have any certain prospect of meeting with any of you. But the confident expectation that I have of meeting my philosophical and christian friends again, is a source of consolation and pleasing reflection in my present state of *exile* from them that is invaluable.

Tho' compared to this, the most important of all subjects, I feel but little interest in the question which, in this treatise, I bring once more before the Public; it is a great satisfaction to me, that I have the sanction of my friends of the lunar society at Birmingham, for the doctrine maintained in this treatise; and notwithstanding the great names among the advocates for the new system in other countries, as well as in France, there are nowhere to be found men of more knowledge, sagacity

\* Since this was written I have heard that one of the members of this society, viz. Dr. Withering, is dead.



gacity, and cool observation, than in your body. No person needs to be ashamed of being in an error in such company.

Assure them all, that I shall ever think of them with particular esteem and affection ; and if, contrary to my present expectations, there should be an interval of *peace* in this most disastrous war, while I am able to bear the voyage, I flatter myself with the prospect of paying a visit to my friends in England ; and then I shall certainly take the first opportunity of attending one more of your meetings. If providence should order otherwise, Adieu till we meet in more favourable circumstances than we can ever do at Birmingham.

With the greatest esteem and affection

I am,

Dear Sir,

Yours sincerely.

J. PRIESTLEY.

*Northumberland, October 20, 1803.*

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## The P R E F A C E.

**D**ESIROUS of bringing the important controversy concerning *phlogiston* to a fair decision, I some years ago, made many experiments with that view, the result of which appeared to myself favourable to the discarded hypothesis. Since my removal to America, where, after a long interruption of my pursuits, I found myself in circumstances tolerably favourable to the resumption of them, one of the first things that I did was to continue the same research, and many of these new experiments being favourable to the old theory, I endeavoured in several publications, especially in the *Medical Repository* printed at New-York, to promote the same discussion; some of the articles being written in direct defence of what I had advanced, and others in reply to objections; and having now, I imagine,

gine, heard all that can be urged in favour of the new theory, from its ablest advocates, both here and in Europe, and thinking it far from being sufficient for its support, I republish in this work all that I think of importance in the former publications, and present it to the public as a *demonstration of the doctrine of phlogiston*, and a complete refutation of that of the *composition of water*. For, after the best attention that I am able to give to the subject, such it appears to me.

The refutation of a fallacious hypothesis, especially one that is so fundamental as this, cannot but be of great importance to the future progress of science. It is like taking down a false light which misleads the mariner, and removing a great obstacle in the path of true knowledge. The longer such an hypothesis has been received, and the more numerous and able are its advocates, the greater service is rendered to science by the refutation. And there is not perhaps any example of a philosophical hypothesis, since the revival of true science, more generally received, or maintained by persons of greater eminence, than this of the rejection of *phlogiston*. In this country I have not heard of a single advocate for phlogiston. In England they are very few, and none of them have written any thing on the subject. In France they are still fewer, and in Germany I hear of no names besides those of Crell, Westrum, Gmelin, and Mayer. No person, however, need to be ashamed of avowing an opinion which has the sanction of such names as these. But what any of them may have written in defence of phlogiston is unknown to me;

so that tho' we are engaged in the same cause, we are unable to give the least assistance to each other.

Removed as I now am to so great a distance from the great theatre of philosophical pursuit, and out of the way of early intelligence (our communications with Europe being also farther interrupted in the present unfortunate state of war) I necessarily labour under various and great disadvantages. I am thankful, however, to a kind providence for the *quiet* that I here enjoy in this remote situation, and for such means of prosecuting my studies, as, considering the state of the country, are very ample.\* And I hope that, considering my advanced age, I shall be thought to have been tolerably assiduous in making use of them.

But my philosophical friends must excuse me if, without neglecting natural science, I give a decided preference to theological studies, and if here, as in Europe, I give the greatest part of my time to *them*. They  
are

\* To the account of my reasons for leaving England I prefixed a motto from Petrarch, whose Latin works, and especially his Letters, often amuse and interest me. It was from his address to his patron the Cardinal of Colonna.

C. Quo fugis? Expecta. Liceat condiscere causas  
Dissidii. Tunostra, puer, nisi fallor, amabas Pascua.

P. Parce, Parens, damnare tuum—Tibi lætior annis,  
Tunc animus fuerat. Nunc intracabilis, asper.

I may now apply to myself what he addressed to the bishop of Cabbassele when he was at Vauluse, absent from his native country Italy.

Exul ab Italia, furis civilibus actus,  
Huc subii, partimque volens, partimque coactus.  
Hic nemus, hic amnes, hic ocia ruris amoeni.  
Sed fidi comites absunt, vultusque sereni.  
Hoc juvat, hoc cruciat. Nihil illis dulce remotis.

are unquestionably of unspeakably more importance to men, as beings destined for immortality ; and I apply myself with so great satisfaction to the study of nature, not so much on account of the advantage we derive from it at present, tho' this is very considerable, as from its being a delightful field of speculation barely opening to us here, and to be resumed with far greater advantage in a future state.

No discovery in philosophy bears any proportion in real value to that of *bringing life and immortality to light*, which is completely effected in the Gospel, and no where else. None of our experiments, or observations on the course of nature, could have given us the least glimpse of this.

But the evidence of this great truth, tho' of the most satisfactory kind, not being that of *sense*, but requiring *attention* and *reflection*, persons much engaged in the business of the world, and even in literary and scientific pursuits, are not always convinced by it. It also requires a candid and well disposed mind, and therefore philosophers (who have their prejudices as well as other men) are not always christians. Among those of this class, I am however, happy in being able to rank not a few, who would do honour to any cause ; and the number of truly philosophical christians, I am well persuaded, will in due time increase. As Paul said to king Agrippa (who said that he had " almost persuaded him " to be a christian") that " he wished that both he, and " all who then heard him, were both almost, and altogether, such as he himself was, except his bonds ; " so there is no greater happiness that I can wish to all

my philosophical friends, than that, with respect to religion and their future prospects, they were what I am, without the calumnies, and the still more serious injuries, to which I have been exposed.

Without a view to this future situation, all our pursuits appear to me to have little in them that is interesting, especially in the decline of life, and the near prospect of death, which, if it put a period to our existence, involves every thing in everlasting darkness, leaving us uncertain whether even the world itself, and the whole race of man, as well as all other animals, may not be doomed to destruction. How gloomy is this prospect, and how dead and indifferent does it render a reflecting mind to every great pursuit !

How thankful, then, ought we to be for an assurance of an endless state of existence, and in circumstances infinitely more favourable than the present. The evidence of this great doctrine (in comparison with which every other inquiry is as nothing) is surely worthy of our most assiduous examination, infinitely more so than a title to an estate, or a claim to a kingdom in this world, which no wise man would be thought justifiable in neglecting.

This being perhaps the last time that I may have an opportunity of addressing myself to my philosophical friends, who, I am concerned to perceive, are generally unbelievers in revelation, I would make it my dying request, proceeding from the most sincere good will to them, to attend to this subject, especially to what I took the liberty to urge in the *Preface* to the sixth volume of my *Observations on air*, which was reprinted in the new edition.



edition of that work in three volumes, and also to my *Letters to the philosophers and politicians of France on the subject of religion*, my *Letters to a philosophical unbeliever*, and my other works in defence of revelation.

Independently of the consideration of the infinitely superior importance of the subject, religion will give a double relish to philosophical pursuits, and will thereby contribute to their success. It is only a wretched superstition, and not religion, that draws men's attention from natural science, or with any other view drives men into retirement, and excludes them from any active and useful pursuits. On the contrary, it tends to inspire men with increased activity, and imparts increasing satisfaction and animation in every proper and laudable exertion. Of this I think I may say I have exhibited an example myself. My numerous publications will shew that from early life I have given the greatest part of my time to theological studies, and yet few have been more assiduous in physical inquiries since I have had the means of doing it. Do not then say that religion makes men idle, or busy to no useful purpose.

Call this, if you please, the talkativeness of age ; but believe it to proceed from a zeal in the best of causes, and sincere good will to yourselves. For I find that I have insensibly got into a direct address in the form of a *dedication*, rather than that of a *Preface*. With this, however, I conclude. Farewell, and may we meet where our present doubts will be removed, and where we shall make more rapid advances in knowledge, without that envy and jealousy, from which philosophers are no more exempt than other men, and which, tho' it has an  
excellent

## THE PREFACE.

excellent effect in making men cautious, and even ardent in their pursuits, from a view to the reputation they hope to acquire by their discoveries, too often makes their pursuits the source of more pain than pleasure to them. Hereafter, we shall, I doubt not, be even more actively employed, and more happy in consequence of it, from better motives.

I shall close this Preface with the Letter I addressed to the advocates for the new theory in France in the first pamphlet I published in answer to them, and also a second, which I address to them in the present state of the controversy.

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*To Messrs. Berthollet, De la Place, Monge, Morveau, Fourcroy, and Hassenfratz, the surviving answerers of Mr. Kirwan.*

GENTLEMEN,

HAVING drawn up a short defence of the doctrine of *phlogiston*, I take the liberty of inscribing it to *you*, as the principal advocates for the Antiphlogistic theory. My view in this is to draw your attention once more to the subject, and I request the  
favour



favour of an answer to my objections. I hope I am not wanting in a proper deference to the opinion of men so justly eminent as yourselves and your friends in France, and also that of great numbers in England, and wherever chemistry is known, who have adopted your hypothesis. But you will agree with me, that no man ought to surrender his own judgment to any mere *authority*, however respectable. Otherwise, your own system would never have been advanced.

As you would not, I am persuaded, have your reign to resemble that of *Robespierre*, few as we are who remain disaffected; we hope you had rather gain us by persuasion than silence us by power. And tho' we are all apt to flatter ourselves, we hope we are as willing to be influenced by the former, as we are inflexible to the latter. If you gain as much by your answer to me, as you did by that to Mr. Kirwan, your power will be universally established, and there will be no *Vendée* in your dominions.

Differing as we do in this respect, we all agree in our wishes for the prevalence of *truth*, and also of *peace*, which is wanted as much for the interests of philosophy, as those of humanity. And on this account I earnestly wish success to the liberty and prosperity of France, which did me the honour to adopt me when I was persecuted and rejected in my native country. With great satisfaction, therefore, I subscribe myself

Your fellow-citizen,

JOSEPH PRIESTLEY.

*Northumberland in America, June 13th, 1796.*

*A second Letter to the same.*

GENTLEMEN,

ABOUT three years ago I took the liberty to request your reconsideration of the doctrine of *phlogiston*, which you had long discarded. A very respectable advocate of your system, Mr. Adet, being then in this country, he replied to my defence of it, and at length I have just received what may be called your definitive answer, in the *Report of Messrs. Berthollet and Fourcroy* on the merits of our performances, in the 26th volume of the *Annales de Chymie*, in which you consider me as supporting a system *un peu chancelante*. As a friend of the weak, I have, indeed endeavoured to give it a little assistance; and as there is no giving strength to one of the opposite systems without taking it from the other, I presume that yours is now in the same situation, calling to you for all the support that you can give to it.

On the opening of this controversy I told Mr. Adet that I should have greater pride in acknowledging myself convinced, if I saw reason so to be, than in victory, and should surrender my arms with pleasure. I was sincere in that declaration; and certainly the conquest of a man's prejudices is more honourable to him than the discovery, or the most successful defence, of any truth.

truth. This, however, I must, for the present at least, decline, and leave it to you ; contenting myself with the inferior praise of confirming the hypothesis for which I have contended. If, from the politeness habitual to Frenchmen, you should decline this honour, thinking my claim to it better founded than yours, I may hereafter be induced to receive it ; but for the present, yielding to you a palm more glorious than that of any victory, and trusting that your political revolution will be more stable than this chemical one,

I am, with the greatest respect,

Gentlemen,

Your fellow-citizen,

J. PRIESTLEY.

*Northumberland in America, Oct. 22, 1803.*

# The P R E F A C E

TO THE  
SECOND EDITION.

**T**HE first edition of this pamphlet was published in the beginning of the year 1800, and it comprized the substance of several proceeding publications on the same subject, after I had heard the observations that had been made on them by the chemists in America and also those in France, the authors of the system that I controvert. I wrote with a view to promote the discussion of a question that is acknowledged to be of very great importance in chemistry, and to which I had not been able to engage much attention; tho' some, and the most material, of my objections had been before the public as soon as it was generally known, and it is now nearly twenty years since this new theory was advanced.

At length, however, I found that I had so far succeeded, that Mr. Cruikshank replied to my pamphlet,  
tho'

tho' to not more than to an argument from one of my experiments, and that the principle on which he replied is greatly approved by the advocates for the new chemistry in France.

To Mr. Cruikshank's animadversions (published in Mr. *Nicholson's Journal* Quarto vol. 5, p. 1) I replied, and on the twenty second of the same month, viz. March 1802, in which my answer was published he dates his reply to it, written with an air of the most perfect persuasion of his superiority in the argument. Immediately on the receipt of this, which (living as I do at a great distance and the communication by sea especially in time of war being slow) could not be very early, I wrote my reply, and it was published by Mr. Nicholson in his *Journal* for Feb. 1803.

In this I expressed great satisfaction in the discussion being entered upon by a person so well qualified to do justice to his argument as Mr. Cruikshank is universally acknowledged to be, and I invited him to discuss the other articles of the difference between the two systems, several of which are of more importance than that which he undertook to canvass.

Ready as he was to reply before, I fully expected his answer in the month following the publication of mine; but having before me five succeeding numbers of the *Journal*, in none of which is there any notice of my last communication by Mr. Cruikshank, or any other person, and there being a demand for a new edition of this pamphlet, I have not thought it necessary to wait any longer; and my answer to his objections, with some little addition, will be found in the fourth section. Still,

however, if I hear any thing farther on the subject, from Mr. Cruikshank, or any other chemist of note, in England or elsewhere, I shall not fail to notice it in some way or other ; and I hope I am not thought to be so much attached to any opinions I have advanced, but that I shall frankly acknowledge any mistake that I may have inadvertently fallen into. Of this, however, our readers will be the proper judges.

I refer the reader to several judicious and well conducted experiments of Dr. Woodhouse, Professor of chemistry in the university of Philadelphia, in an *Appendix to the American edition of Parkinson's Chemical pocket book*, on the calces of several of the metals, the results of which are manifestly inconsistent with the principle of one part of the new theory, which the ingenious experimenter consequently abandons. In my opinion the abandonment of any of its parts must lead to that of the whole, all the parts of it having the closest connection with each other ; but the perception, and acknowledgment of this, must be the work of time and reflection.

Dr. Woodhouse clearly proves that the air that is procured from charcoal and water will by no means warrant the conclusion that Mr. Lavoisier draws from it, and that my inference from it is just, p. 209. He shews that water is the principal agent in producing part of the inflammable and fixed air that is got from charcoal and the calces of several of the metals, especially that from finery cinder ; that “ if the oxygen  
“ in them was the sole agent, the inflammable air pro-  
“ cured from them in these circumstances could not be  
“ obtained



“ obtained ; and that the flowers, or white oxide of zinc, frequently affording inflammable and no fixed air, when subjected to heat with coal, cannot be accounted for by the new doctrine.” p. 216.

But if the calces of these metals, especially that of iron, be not perfect oxides, the proof of the *decomposition of water* must fall to the ground ; and there, I am now pretty confident, it lies, and that all efforts to raise it will be as ineffectual as those of Mr. Cruikshank.

My labours of every kind now rapidly drawing to a close, I propose soon to reprint several articles of observations and experiments that are published in the *Transactions of the Philosophical Society in Philadelphia*, in *Mr. Nicholson's Journal*, and the *Medical Repository* of New York, and a few other articles not yet published. These, together with this tract in defence of the doctrine of phlogiston, will make a seventh volume of my philosophical writings, or a fourth to those that are abridged and methodised in three, and they complete all that I have been able to do for the advancement of natural science.

Considering the many, and sometimes long, interruptions of my labours, especially in consequence of removals, as from Warrington, where I began my experiments in *electricity*, to Leeds, where I began those on the *different kinds of air* ; from Leeds to Wiltshire, when I spent the winters in London, and during which time I accompanied the Marquis of Lansdown in a tour through Flanders, Holland, part of Germany, and France ; from Wiltshire to Birmingham, where all my philosophical

philosophical apparatus was destroyed ; from Birmingham to Hackney, where my stay was too short to admit of my doing much business of any kind ; and lastly from Hackney to this country ; and here, so often from one house to another (the inconvenience and loss of time attending which only an experimenter can form an idea of) together with many disadvantages under which I now necessarily labour, where almost every thing relating to experimental philosophy and chemistry must be had from Europe ; when, I say, these circumstances are considered, and that I did not (for want of opportunity) make any original experiments till I was near forty, I hope it will be thought that I have not been deficient in point of *industry*, whatever may be thought of the value of my labours.

I am truly thankful to the sovereign disposer of all things, and to those friends of science who have defrayed the expenses of my laboratory, that I have been able to do so much ; and I hope my liberal benefactors will not repent of their generosity. Indeed, the greater part of them are gone before me, to a state in which I shall hope to join them, where I can again express my gratitude, and when we may again unite our respective abilities in the same pleasing pursuits ; seeing more of the wisdom and goodness of the great creator, and having our admiration and devotion excited to a greater height than ever.

The



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E R R A T A.

- PAGE 29 l. 9, *after* unite, *add* with the carbone  
— 41 l. 17, *dele* of  
— 102 l. 5 *from the bottom after* procured,  
*add* by means of water.  
— 108 Note l. 8, *after* upon, *add* them



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THE  
DOCTRINE OF PHLOGISTON  
*ESTABLISHED.*

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THE INTRODUCTION.

THERE have been few, if any, revolutions in science, so great, so sudden, and so general, as the prevalence of what is now usually termed the *new system of chemistry*, or that of the *Antiphlogistians*, over the doctrine of Stahl, which was at one time thought to have been the greatest discovery that had ever been made in the science. I remember hearing Mr. Peter Woulfe, whose knowledge of chemistry will not be questioned, say, that there had hardly been any thing that deserved to be called a *discovery* subsequent to it. Tho'  
A there

there had been some who occasionally expressed doubts concerning the existence of such a principle as that of *phlogiston*, nothing had been advanced that could have laid the foundation of *another system* before the labours of Mr. Lavoisier and his friends, from whom this new system is often called that of the *French*.

This system had hardly been published in France before the principal philosophers and chemists of England, notwithstanding the rivalry that has long subsisted between the two countries, eagerly adopted it. Dr. Black in Edinburg, and, as far as I hear, all the Scots have declared themselves converts, and what is more, the same has been done by Mr. Kirwan, who wrote a pretty large treatise in opposition to it. The English reviewers of books, I perceive, universally favour the new doctrine. In America also, I hear of nothing else. It is taught, I believe, in all the schools on this continent, and the old system is intirely exploded. And now that Dr. Crawford is dead, I hardly know of any person, except my friends of the Lunar society at Birmingham, who adhere to the doctrine of phlogiston.

It is no doubt *time*, and of course opportunity of examination and discussion, that gives stability to any principles. But this new theory has not only kept its ground, but has been constantly and uniformly advancing in reputation about *fifteen years*, which, as the attention of so many persons, the best judges of every thing relating to the subject, has been unremittingly given to it, is no inconsiderable period. Every year of the last twenty or thirty has been of more importance to science, and especially to chemistry, than any ten in the preceding century.

ture. So firmly established has this new theory been considered, that a *new nomenclature*, entirely founded upon it, has been invented, and is now almost in universal use; so that, whether we adopt the new *system* or not, we are under the necessity of learning the new *language*, if we would understand some of the most valuable of modern publications.

In this state of things an advocate for the old system has but little prospect of obtaining a patient hearing. And yet, not having seen sufficient reason to change my opinion, and knowing that free discussion must always be favourable to the cause of truth, I wish to make one appeal more to the philosophical world on the subject. Besides having many new observations to advance, I cannot help thinking that what I have observed in several of my former publications has not been sufficiently attended to, or well understood. I shall, therefore, endeavour to bring into one view what appears to me to be of the greatest weight, avoiding all extraneous and unimportant matter; and perhaps it may be the means of bringing out something more decisive in point of *fact*, or of *argument*, than has hitherto appeared.

No person acquainted with my philosophical publications can say that I appear to have been particularly attached to any hypothesis, as I have frequently avowed a change of opinion, and have more than once expressed an inclination for the new theory, especially that very important part of it *the decomposition of water*, for which I was an advocate when I published the sixth volume of my *Experiments on Air*, though farther re-

reflection on the subject has led me to revert to the creed of the school in which I was educated, if in this respect I can be said to have been educated in any school. However, whether this new theory shall appear to be well founded or not, the advancing of it will always be considered as having been of great importance in chemistry, from the attention which it has excited, and the many new experiments which it has occasioned, owing to the just celebrity of its patrons and admirers.

In matters of much nicety, as the subjects of many of my numerous experiments are, I do not always expect to escape the charge of inaccuracy, and perhaps of inconsistency. Persons who, from a want of experience, are not sufficiently aware of the difficulties, will not have the candour that the circumstances call for. From such I must appeal to the judgment of those who have the requisite experience and qualifications. I will, however, venture to say, that no person who has made near so many experiments as I have, has made so few mistakes. I do not mean with respect to *opinions*, but in my reports of *facts*. But after all our care, errors will sometimes arise from a want of attention to small differences of circumstances; and no person can keep his eyes open to every thing that is before him at the same time.

SECTION



## SECTION I.

*That Metals are compound Substances, and contain Phlogiston, proved from the Solution of Iron in the vitriolic and marine Acids, and from some other Considerations.*

**A**CCORDING to the doctrine of phlogiston, advanced by Becher and Stahl in the beginning of this century, and much simplified and improved since their time, metals, phosphorus, sulphur, and many other substances which are supposed to contain it, are compounds, consisting of this principle, and another which may be called its *base*. Thus each of the metals contains phlogiston united to a peculiar calx, and sulphur and phosphorus consist of the same principle and their respective acids, or the bases of them. But according to the antiphlogistic theory, all the metals are simple substances, and become calces by imbibing pure air; and sulphur and phosphorus are also simple substances, and become the acids of vitriol and of phosphorus by imbibing the same principle, called by them *oxygen*, or the principle, as it probably is, of universal acidity.

And whenever inflammable air is procured by means  
of

of any *metal*, they say that it does not come from the *metal*, but from a decomposition of the *water* that is present, and which they say consists of two elements, viz. *oxygen*, or the base of dephlogisticated air, and *hydrogen*, or the base of inflammable air, in the proportion of eighty five parts of the former, to fifteen of the latter.

1. The most simple of the experiments that I have proposed for discussion, with a view to decide concerning the merits of these two theories, and which I cannot help thinking furnishes an argument no less than *demonstrative* of the fallacy of the antiphlogistic hypothesis, is that of the solution of iron in the vitriolic and marine acids. Here the question to be solved is, from which of the substances present comes the *inflammable air* that is procured in the process. The phlogistians say it comes from the *iron*, and the antiphlogistians from the *water*. But to this I object that, since, according to their own hypothesis, water consists of about six times as much oxygen as it does of hydrogen, there must be a large deposit of oxygen in the vessel, and that I cannot find it there. That it is not in the *acid* appears, as the antiphlogistians themselves say, by its saturating no more alkali after the process than before. They, therefore say (and there is no other alternative) that this addition of oxygen is in the *iron*.

But I ask, how does this appear? If there be any addition of oxygen in this case, it must shew itself either by an addition to the acid, or by its being exhibited in the form of dephlogisticated air, called by them *oxygencous gas*. The former is not pretended, and so far

far is the latter from being true, that if the precipitate be exposed to a red heat, it yields much less pure air than the same quantity of the acid, without the iron, would have done.

For this purpose I took as much vitrolic acid as I had found in the experiment recited in Vol. iii, p. 197, of my *Observations on Air* (in three vols.) to have yielded one hundred and thirty ounce measures of dephlogisticated air, of the standard of . 15, which is extremely pure, and saturated it with iron. But after this it yielded only fifty two ounce measures of air, of the standard of . 55, which is much less pure. This shews that this precipitate is so far from containing more oxygen, that it contains less than the acid. It is in reality possessed of the opposite principle, which is agreeable to the phlogistic theory. For since much more inflammable air is procured from iron by means of steam only, than by its solution in any acid, more of the principle of which inflammable air consists, viz. phlogiston, must adhere to this calx of iron than to the other.

Dr. Maclean says p. 19, “ There is the most satisfactory evidence that iron, after its solution in sulphuric acid is in a state like that of the black oxyd, or “finery cinder.” But the dephlogisticated air that is yielded by this precipitate is all procured before it comes to this form of a calx. After it becomes black, in which state it ought to contain more oxygen in proportion to its bulk than before, it yields no oxygenous gas at all. Also, neither in this, nor in any other state, will it oxygenate muriatic acid, as *minium*, and some other substances which contain dephlogisticated air, do, which

however easily dissolves it. It, therefore, shews no sign of its containing any oxygen at all. The new theory, however, requires that it be dignified with the appellation of the *black oxyd of iron*. The black oxyd of *manganese* gives more evidence of its right to the name they have given to it, tho', according to them, it contains much less oxygen. It is evident, therefore, that there is no addition of oxygen in this process, consequently no decomposition of water in the case; and that the inflammable air must come from the decomposition of the iron.

I have no great objection to admitting that this precipitate from the solution of iron in the vitriolic acid, when it is burned black, is the same substance with finery cinder. But this will appear to be no advantage to the antiphlogistic theory. Both in this form, and in that of a brown powder, this precipitate has several of the same properties with those of finery cinder. They neither of them either gain or lose any weight by exposure to the greatest heat. When heated in atmospheric air, they both diminish, and, as I usually say, they phlogisticate, it, though very slowly. They also equally imbibe inflammable air when heated in it, but with this difference, that the production of water seemed to be greater in the reduction of finery cinder than in that of this precipitate. But the experiment being of no great consequence, I did not give much attention to this circumstance.

If this black calx that remains after heating this solution of iron in the acid contain all the oxygen both of the water expended, and of the deficiency of acid

acid in the solution, it must be doubly oxygenated, containing more oxygen than an equal quantity of common finery cinder ; a difficulty of which, I imagine, the advocates for the new theory were not aware.

There is something extraordinary in the manner in which the antiphlogistians suppose that metals become soluble in acids. Mr. Adet says, p. 60, “ Experiments “ prove that metals, in order to be combined with an “ acid, require to be united with oxygen ;” and explaining himself farther, he says, “ In reality, a metal not “ combining with acids but when it is in a state of oxide, and not passing into this state but by its union “ with its oxygen, must necessarily absorb oxygen in “ order to unite with the acid. But this oxygen can only be supplied by one of these two substances, the “ acid, itself, or the water which it contains. If the “ oxygen had been given by the acid, it would have “ been in part decomposed, and would in consequence “ have saturated less alkali. But since it saturates the “ same quantity of alkali, it has not been decomposed.”

On this I would observe, that if the separation of the oxygen from the water, in order to its attaching itself to the iron, take place prior to its solution in the acid, that solution is not necessary to its producing inflammable air ; for if the oxygen of the water be seized by the metal, the hydrogen of the water must escape in the form of inflammable air ; and this effect would in all cases be produced by some affinity between the iron and the oxygen in the water only.

If the affinity be between the iron and the oxygen universally, what could prevent the iron from saturating

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ing itself in the first instance with that which belongs to the acid, as well as with that which was a constituent part of the water, in which it is at least much less evident. I would also ask, if an acid will not dissolve iron till it be oxydated, but will do so when it is, why will not the acid of vitriol dissolve the black oxyd of iron, or fine-ry cinder, more readily than it does iron ; since in this substance it finds the iron already abundantly oxydated ; and yet the reverse of this is the case.

2. Inflammable air is procured when one metal is precipitated from its solution by another in its metallic state. This is a fact that is very easily explained on the supposition that the metal precipitated does not require so much phlogiston as that which is dissolved ; but the doctrine of the decomposition of water cannot, as far as I see, account for the fact, at least in an easy and natural way.

When zinc is used to precipitate lead from a solution of sugar of lead, inflammable air is procured ; and according to the phlogistic theory it ought to be so ; since lead contains much less phlogiston than zinc, so that when the former is revived by means of the latter, it is able to furnish more than is requisite for the purpose. But if this inflammable air came from the decomposition of the water, the oxygen, which must be developed at the same time, ought to be found either in the water, or in what remains of the zinc. For it will not be pretended to be in the *lead* that is revived, and there are no other substances present.

Iron, I also find, will yield more inflammable air by solution in acids than zinc ; and a saturated solution

of iron in the marine acids yields inflammable air by the solution of zinc.

To the arguments in this section to prove that metals are compound substances, and contain phlogiston, I shall add the following.

My experiments prove to demonstration that nitrous acid is wholly composed of dephlogisticated and nitrous air ; since when they form this acid, they unite without any residuum, or so small as not to enter into any computation. Had there been any phlogisticated air in either of these component parts of the acid, it would have appeared on their uniting, and thereby losing their aerial state. For as neither of them will unite with it, it must then have appeared in its proper form. If, therefore, in any process phlogisticated air be formed by means of nitrous air, one essential ingredient in the constitution of that air must come from another source; and all that can be said is that the nitrous air furnished one component part of it.

But phlogisticated air is produced by heating iron in nitrous air. Something, therefore, must come from the iron in order to form it, and consequently iron can not be a simple substance ; and if iron be a compound, it will not be questioned but that other metals must, from analogy, be compounds too ; and since nitrous acid can be formed by means of both inflammable and phlogisticated air, the same principle which is denominated *phlogiston*, must enter into them both.

## SECTION

## SECTION II.

*Of Finery Cinder.*

THE great question between the advocates for phlogiston and their opponents is, whether the substance that has usually been called *finery cinder* (which is formed by the contact of steam with iron when it is red hot) be a proper *oxide of iron*, that is, whether it contain any principle which can be exhibited either in the form of an acid, or of dephlogisticated air; and yet this, which is the only proper evidence in the case, has not been given. To say that it forms *water* when heated in inflammable air, and that water cannot be formed without oxygen, is taken for granted the very thing to be proved; since the water so procured, I say, is that which was imbibed by the iron, and is now expelled on the introduction of the phlogiston with which it had parted.

One of my arguments to prove that finery cinder contains no oxygen is, that when it is dissolved in marine acid, it does not oxygenate it. Let us, however, hear the account that my opponents give of this circumstance. Mr. Adet says p. 55, "The nonoxygenation  
" of



“ of the muriatic acid by the solution of finery cinder,  
 “ is owing to the latter retaining the oxygen so strongly,  
 “ as not to be disengaged by the action of heat, aided  
 “ by the attraction of the muriatic acid.” To this I  
 answer, that if the acid had not been able to dissolve  
 this substance, this might have been said with some de-  
 gree of plausibility ; but since it does dissolve it com-  
 pletely, so volatile a thing as oxygenous gas, of which  
 it is supposed to contain so large a quantity, and, with  
 which this acid has so strong an affinity, could hardly  
 escape being evolved.

Messrs. Berthollet and Fourcroy say that “ finery  
 “ cinder, like massicot, is unable to dephlogisticate ma-  
 “ rine acid, because it contains no more oxygen than is  
 “ necessary to its solution ; whereas the metals that  
 “ have got a greater proportion of oxygen, give out  
 “ what they have that is superabundant to a part of the  
 “ muriatic acid, which by that means becomes oxyge-  
 “ nated” (*Annales de Chymie*, Vol. 26, p. 305) evi-  
 dently taking it for granted, that finery cinder, like mas-  
 sicot, contains but little oxygen, whereas, if it contain  
 any, it must be much more than any other substance in  
 nature.

Dr. Maclean makes very light of this, as indeed he  
 does of every other difficulty. “ It certainly” he says  
 p. 10, “ does not follow that because muriatic acid can  
 “ separate a certain quantity of oxygen from lead, when  
 “ this is combined with a great quantity of that sub-  
 “ stance, that it should likewise separate oxygen from  
 “ iron, when this is united to a comparatively small  
 “ quantity.” But finery cinder, if, as all antiphlogisti-  
 ans

ans say, it owes all its additional weight to pure oxygen, which it gained from the water which it had decomposed, must contain much more of it than lead in any state. For the addition to its weight is nearly one third; whereas the addition to the weight of lead by converting it into minium, is only about one tenth of its weight. Can this be all pure oxygen that the iron acquires, and yet not oxygenate muriatic acid?

He farther says p. 24. "The antiphlogistians suppose the addition made to iron to be by oxygen, because the compound resembles in every respect, as as Dr. Priestley himself allows, that substance which is formed by burning iron in oxygenous gas, or in atmospheric air. And this they consider as an oxyd, because while it is forming the oxygenous gas disappears, and its weight is exactly equal to that of the oxygen and iron consumed."

But it is evident to me, that though the pure air or oxygen disappears in this process, it is not imbibed by the *iron*, but only the *water* which was its base, and which formed at least the principal part of its weight; the pure air, or oxygen, serving to form the *fixed air* which is always found in this process, and which cannot have any other origin. Consequently, the calx of iron so formed, when heated in inflammable air, gives out nothing but water. The quantity of fixed air produced in this process appears to me to be quite sufficient to take all the pure air that disappears in it. It is possible, however, that a small quantity of oxygen may enter the iron along with the water to which it was united; as  
few

few substances are perfectly separated from each other by any chemical affinity.

When the spirit of salt is distilled over a quantity of scales of iron, which, being made in the open air, are most likely to have some of this principle attached to them, it has something of that faint smell which a very small quantity of dephlogisticated air will give it. But it is the more evident from this circumstance, that if this species of finery cinder had contained any considerable quantity of oxygen, it would have been extricated in this process. That a little, and not more, appeared, I consider as a proof that it contained no more ; whereas, according to the new theory, it must contain more than any other substance.

That a very small quantity of oxygen is attached to the scales of iron, I have thought probable from a barely perceivable quantity of fixed air which I have sometimes found when they are revived in inflammable air. But so small a quantity as this makes nothing for the new theory.

That finery cinder does not dephlogistificate marine acid is, I acknowledge, no absolute proof that it contains no oxygen ; because this effect is not always produced by red precipitate, which is known to contain a great proportion of oxygen, nor by flowers of zinc, or massicot, which, I doubt not, contain some. On the first pouring of marine acid on red precipitate fresh made, I have had an evident smell of dephlogisticated marine acid, but not afterwards. Also, the black powder of mercury and lead, which gives pure air by heat, does not dephlogistificate marine acid, tho' it makes it give an offensive

sensitive smell. But if it be considered how much more oxygen, according to the antiphlogistic theory, is contained in finery cinder than in any other substance, it will appear to amount to little less than a demonstration of its containing none, that it has not this effect. From an ounce of red precipitate, or of minium, about sixty ounce measures of dephlogisticated air may be expelled by heat, which is not more than about a thirtieth part of their weight. But if all the addition gained by iron, when it is converted into finery cinder, be pure oxygen, it amounts, as I have observed, to near one third of its weight; which is almost ten times more than is contained in either of the other substances.

Besides, there is other evidence of all these substances containing oxygen, not only when exposed to heat, but, with respect to the red precipitate, when dissolved in marine acid; and there is no evidence of any kind that finery cinder contains this principle.

The solution of red precipitate, heated with a burning lens in atmospherical air, causes an addition to its quantity, from the dephlogisticated air expelled from it; whereas, when the solution of finery cinder is treated in the same manner, the contrary effect is produced. The quantity of air is diminished, and the remainder is less pure than before. The same was also the consequence of heating the solution of iron in the same circumstances, that of finery cinder precipitated by caustic volatile alkali, and of iron itself treated in the same manner.

Since, therefore, finery cinder, both in this solution and without it, has the same effect on the atmospherical  
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air in which it is heated that iron has, I conclude that they contain the same principle, tho' the finery cinder has much less of it than the iron. The same is probable from finery cinder being in some degree attracted by the magnet. So far, therefore, is finery cinder from containing any oxygen, that it contains some of the opposite principle.

Another probable evidence of a calx containing oxygen, or dephlogisticated air, is that when it is revived in inflammable air, fixed air is produced. But this is not the case when finery cinder is revived in these circumstances, tho' I purposely prepared some by melting iron in the open air, in which case I had imagined that some pure air would be attached to it. \*

Since an iron tube is dissolved by heating manganese in it, I thought it very possible that some dephlogisticated air from this substance might unite with the iron, and therefore that the finery cinder made in this manner might be found to contain some. But when I heated some iron affected in this manner in inflammable air, I did not find any fixed air in the residuum; so that it appeared to have got nothing but water from the manganese, being the same thing with the finery cinder made by means of steam.

Because the calx of mercury derives its additional weight from dephlogisticated air, the antiphlogistians have too hastily concluded that all metallic calces derive

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their

\* In making this finery cinder I observed that steel gained no sensible addition of weight in the process, much less than when it is made by means of steam in a close vessel. When it was procured in a glass receiver standing in water, it gained some weight; but when it was done over mercury, the addition to its weight was little or nothing.



their additional weight from the same cause. But this is not by any means a just inference. For the calces of some metals are, in this and other respects, very different from one another, and even the different calces of the same metal.

Finery cinder, for example, is a very different thing from the common *rust of iron*, consisting of different principles. From finery cinder nothing can be got by mere heat, but from the rust of iron a large quantity of fixed air is got in the same process. From one thousand two hundred and seventy seven grains of rust, I got forty five ounce measures of air, of which only about one thirtieth part was not fixed air.

The addition that is made to iron by rusting in the open air, I do not find to be more than thirty or forty grains to an ounce ; whereas the addition to an ounce of iron when it is converted into finery cinder is about two hundred grains.

What makes it almost a certainty that the water which is found on the revival of finery cinder in inflammable air has not the source that the antiphlogistians suppose, is the great difference in the quantity which is found in this case, and that of the revival of other calces in it. Dr. Maclean says, p. 11, "When oxyd of mercury is reduced in hydrogen gas, that disappears, no oxygen gas is obtained, but a quantity of water may be collected." Now I am confident that no person who had ever seen the experiment could have written this. The quantity of water that appears in this case is barely perceivable, being no more than sufficient to constitute the base of the inflammable air imbibed by the calx,

calx, or that might have been concealed in the substance operated upon ; whereas when finery cinder is revived in the same circumstances, the water forms itself into hundreds of small drops, which unite, and run down the inside of the vessel in all directions.

Now if this water was really formed by the union of inflammable air in the vessel with the oxygen expelled from the calx, they ought, surely, to unite in the same proportions, in order to form the same thing. The antiphlogistians themselves always say that the proportion of hydrogen and oxygen in water, is universally fifteen parts of the former to eighty five of the latter. Here, therefore, is much more water produced than their principles can account for. The same quantity of inflammable air disappears, but the same quantity of water is by no means formed. The obvious conclusion therefore is, that in the case of the calx of iron, the great quantity of water produced was simply expelled from the calx when the inflammable air was imbibed ; whereas the calx of mercury contains little or no water to be expelled, and only unites with the phlogiston in the inflammable air that disappears. It will, however, be shewn that it does not always form any union with the inflammable air, but remains mixed with it, so as to occasion dangerous explosions.

Mr. Lavoisier and his associates observe (*Report*, p. 300) that when a calx is revived in inflammable air, more water is found in the vessel than the weight of the inflammable air that disappears, so that it could not have been contained in that air.



In this they only refer to my experiments in general; but as they speak of the water produced as appearing both on the inside of the vessel, and on the surface of the mercury, it can be no other than the experiment of the revival of iron from finery cinder; and the water that is found in this process was never supposed by me to come from the little that is contained in the inflammable air, but from the much greater quantity contained in the cinder.

Before I conclude this section concerning finery cinder, I must take notice of what Dr. Maclean too confidently advances about it. "The Doctor," he says p: 26, "is certainly mistaken in supposing that finery cinder cannot rust. Mr. Fourcroy says it rusts sooner than common iron, and every apothecary knows it does so. If the rust of iron be made red hot in a retort, a quantity of carbonic acid is disengaged from it, and the iron remains in a state of black oxyd. The rust therefore is a *carbonate of iron*, and must contain all the principles which compose the black oxyd, and therefore can contain nothing capable of excluding that which would convert it into rust." But, in direct contradiction to what he asserts, I still say that finery cinder is not subject to rust. In England no use having been made of it before it was attended to by my brother-in-law Mr. John Wilkinson (one of the most intelligent and successful of all the iron-masters in that or any other country) but to mend the roads, it has lain in heaps for years, I may even say ages, without acquiring the least tinge of brown. All my specimens have ever remained free from rust, and the physicians, who are  
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also apothecaries in this place, assure me they never saw or heard of any such thing. They get it from the blacksmiths in the form of *scales of iron*, and the blacksmiths say the same. It must therefore, as I have observed, be saturated with some principle very different from that of the common rust of iron, and is by no means the same thing, notwithstanding what Dr. Maclean says to prove the contrary. If finery cinder be ever converted into rust, which I have never found to be the case, it must, by some process or other, natural or artificial, have been first converted into iron, in which case it must lose much of its weight.

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### SECTION III.

*Of inflammable Air from Finery Cinder and Charcoal.*

IF inflammable air, or hydrogen, be nothing more than a component part of water, it could never be produced but in circumstances in which either water itself, or something into which water is known to enter, is present. But in my experiments on heating finery

cinder together with charcoal, inflammable air is produced, though, according to the new theory, no water is concerned. According to this theory, finery cinder, called the *oxide of iron*, consists of nothing besides iron and oxygen; and the charcoal made with the greatest degree of heat that can be applied, is equally free from water; and yet when these two substances are mixed together, and exposed to heat, they yield inflammable air in the greatest abundance.

This fact I cannot account for on the principles of the new theory; but nothing is easier on those of the old. For the finery cinder containing water, as one of its component parts, gives it out to any substance from which it can receive phlogiston in return. The water, therefore, from the finery cinder uniting with the charcoal makes the inflammable air, at the same time that part of the phlogiston from the charcoal contributes to revive the iron. Inflammable air, of the very same kind is procured when steam is made to pass over red hot charcoal.

Since inflammable air, and in great quantity, is procured in this process, the antiphlogistians are under a necessity of finding *water*, by the decomposition of which, and in no other way, they say it is made; and some of them find it in the charcoal, and others in the finery cinder.

As Dr. Woodhouse repeated this experiment with peculiar exactness, I shall copy his account of it from the *Philosophical Transactions of Philadelphia*, Vol. iv, p. 464. “ An ounce of the scales of iron, and the same  
“ quantity

“quantity of charcoal, were reduced to a very fine  
 “powder, and exposed separately in covered crucibles  
 “in an air furnace well supplied with fuel for five hours.  
 “They were then taken out of the fire, and mixed  
 “while red hot, in a red hot iron mortar, were tirtura-  
 “ted with a red hot pestle, formed of an iron ramrod,  
 “were poured upon a red hot sheet of iron, and instant-  
 “ly put into a red hot gun barrel, which was fixed in  
 “one of Lewis’s black lead furnaces, and which com-  
 “municated with the worm of a refrigeratory, a part  
 “of a hydropneumatic apparatus. Immediately after  
 “luting one end of the gun barrel to the worm, one  
 “hundred and forty two ounce measures of inflamma-  
 “ble air came over in torrents, mixed with one tenth  
 “part of carbonic acid gas.”

Nothing more could have been done to exclude all water from each of the substances previous to their mixture; and yet we immediately find the effects of water as much as if water itself had been employed instead of the finery cinder, which no doubt, contained it. This experiment I should have expected, might have converted the ingenious author of it himself. His explanation of it however, is so unsatisfactory that I cannot help thinking the consideration of it will go a great way towards the conversion of others. For he admits that there really is *water*, and in this great quantity, in the finery cinder.

But if we suppose finery cinder to contain water, and so much of it as is necessary to form all the air that is produced in this process, both fixed and inflammable, we must, surely, abandon the most fundamental princi-

ple of the new theory, which absolutely requires water to be decomposed in passing over hot iron, the oxygen alone remaining in the iron, and the hydrogen escaping in the form of inflammable air; and it is only by comparing the addition of weight acquired by the iron in this case, that the proportion between the oxygen and the hydrogen in the composition of water is ascertained. Besides, how can it be supposed that water should both be decomposed, and not decomposed, in the same circumstances?

To the experiment with the finery cinder and charcoal Mr. Berthollet objects, *Report*, p. 15, that “ I probably got more fixed air than inflammable, that the inflammable air contains much charcoal dissolved in it, and that in many experiments charcoal appears to retain water very obstinately.”

How obstinately charcoal retains water is easily ascertained. When water only *adheres* to any substance without entering into it as a constituent part, a degree of heat capable of converting it into steam, will always be sufficient to expell it; and the antiphlogistians have not yet said that water is an essential part of this *carbone*. This they suppose to be a simple substance; and tho’ common charcoal is not pure carbone, they do not pretend to say that water can be in it except as an extraneous substance. Perhaps when they find their theory *un peu chancelante*, they may have recourse to this support.

Messrs. Berthollet and Fourcroy, however, say that this inflammable air comes from the decomposition of the “ water contained in the charcoal, and which they  
“ say



“ say cannot be separated from it but by forming a new  
 “ combination with it.” *Annales de Chymie*, Vol. xxvi,  
 p. 306.

But as water is no constituent part of charcoal, it certainly may be separated from it by heat, without forming any new combination, or undergoing any decomposition.

If it be the water adhering to the charcoal that is decomposed, and the component parts of this water enter into a new combination with the carbone of it, I ask of what use is the finery cinder in the process, which, however, is essential to the success of it; and why might not the same heat have the same effect in decomposing this water, without the finery cinder, as well as with it?

They do not say they have any occasion for the oxygen contained in the finery cinder, which, however, leaves it in this process; since the iron is revived; and how can they account for the separation of this oxygen from the iron without the supposition of something going in to take its place. Heat alone will not effect this. For heat tends to unite, and not to separate them.

In whatever manner this water, adhering to the charcoal, contributes to the formation of inflammable air, Mr. Berthollet himself would say, that when any particular degree of heat would not make charcoal yield any more inflammable air, there was no more water retained in it than the same degree of heat was able, with its assistance, to decompose. But after this, by the assistance of finery cinder, with even a much less

degree of heat, it yields inflammable air very copiously, just as if steam had been made to pass over it in that heat ; and, judging from evident appearances, there cannot be a doubt but that, with a sufficient quantity of finery cinder to supply it with water, all the phlogiston in the charcoal, exclusive of that which contributed to the revival of the iron, would be converted into inflammable air.

As to the proportion between the fixed and inflammable air procured by this process, it is about the same with that procured from charcoal by means of steam, and will probably vary with the proportion of finery cinder, as that does with more or less water.

That finery cinder contains nothing but water, appears not only from its enabling charcoal to give out air exactly as water would do, but from doing the same with respect to *terra ponderosa aerata*, which also gives out air by means of water, but not without.

I mixed a quantity of this substance, reduced to a powder, with pounded finery cinder, and in a gun barrel, heated red hot, I got from it fixed air as copiously as if steam had passed over it. There was a considerable residuum of inflammable air from the iron.

When I first made this experiment with charcoal and finery cinder, I remember Mr. Watt said, it was one that the Antiphlogistians could never reconcile to their hypothesis ; and the more I consider it, and the objections that have been made to it, the more reason I see to be of his opinion.



## SECTION IV.

*Of Mr. Cruikshank's Hypothesis to account for the Formation of inflammable Air in the Process of heating finery Cinder and Charcoal.*

THE two answers to my argument in favour of the doctrine of phlogiston considered in the preceding section go upon the proper principle of the new theory, viz. that whatever is *inflammable* in any kind of air must come from *water*. But Mr. Cruikshank, seeing, no doubt, the impossibility of finding, or decomposing, water in the case, replies upon a principle intirely new; since he requires no water at all for the formation of the very great quantity of inflammable air that is produced in this process. He even maintains that this air, tho' it be evidently as inflammable as that from charcoal and water, is an *oxide*, calling it the *gazeous oxide of carbone*, supposing it to be formed by the union of oxygen from the finery cinder and carbone in the charcoal, but in a very circuitous way. For he supposes, in the first instance, that a quantity of fixed air is formed by the oxygen of the finery cinder uniting with carbone in the charcoal, and that this fixed  
air

air is decomposed by one of its constituent parts (viz. its oxygen) going back into the finery cinder, and re-converting it into iron ; which iron he says, by means of heat, decomposes the fixed air, so that part of its oxygen can unite with the carbone in the charcoal. Thus, he says, will be formed this *gazeous oxide of carbone* without any water.

This hypothesis, strange as it appeared to me, and even inconsistent with the most fundamental principle of the new theory, I found to be quite satisfactory to the original authors of this theory in France. One of the most eminent of them, in conversation with a friend of mine at Paris, said that "I had kept them for some time in torture by my objections to their system, but that they were intirely relieved by Mr. Cruikshank." In consequence of this information, I have given more attention to this hypothesis than many persons will probably think to have been necessary, and I have replied by stating the following objections to it, each of which I cannot help thinking abundantly sufficient to refute it.

1. Mr. Cruikshank's hypothesis requires that, in the process of heating finery cinder and charcoal, the oxygen in the finery cinder should quit that substance, and unite with carbone in the charcoal, in order to form fixed air. Since, however, this fixed air is to be decomposed by iron, the oxygen which it has got from the finery cinder must be separated from it, and enter into the same calx again, for there is nothing else to receive it. But while the heat continues the same, I deem these contrary effects to be impossible. If the degree  
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of heat that is applied expel oxygen from the calx, it will certainly prevent its return. Consequently, if fixed air could be *formed*, it cannot be *decomposed* in these circumstances.

2. I see no occasion that Mr. Cruikshank had for the formation of this *fixed air*, which, after all, is merely *hypothetical*, as it cannot be exhibited in any part of the process. Why could not the oxygen in the finery cinder unite, in the requisite proportion, to form the partial oxide required? For the hypothesis does not admit of a complete oxidation, since then it could not take more on being fired with dephlogisticated air. The finery cinder can only take from the fixed air the very same thing that it gave out to form it.

3. If it was possible for oxygen to be separated from finery cinder without any thing entering into it (which I think cannot be done) it would not, according to the new theory, form any thing by this union with carbone besides *fixed air*; this being said to be *carbone dissolved in oxygen gas*. Of this Mr. Cruikshank is sensible, and therefore he was under the necessity of supposing that, after the oxygen had quitted the finery cinder, it should in part enter into it again. But if this were possible, nothing would remain of the fixed air but *carbone*, as before that union. For the oxygen that was left in it could only unite with the same proportion of carbone, and this is always a *solid substance*, incapable, without the aid of oxygen gas (or dephlogisticated air) of assuming the form of *air*. Whence, then, comes the *inflammable air* in this process, which so nearly resembles that from charcoal

coal and water, that they must of necessity have the same origin? And with respect to *this*, Mr. Lavoisier decides that, whatever is *inflammable* in it must come from *water*.

4. Admitting all that Mr. Cruikshank alleges concerning the difference in specific gravity, and other circumstances, between the air from finery cinder and charcoal, and that from water and charcoal, it is not so great as the difference between the latter and the light inflammable air from metals with acids or water. Different as they may be in other respects, they are all *inflammable*, having the common property of uniting with oxygen in a certain degree of heat; in consequence of which they are alike the very reverse of *oxides*, and must be classed with *combustible* substances, equally with sulphur and phosphorus.

If this kind of air was a real *oxide*, it would appear to be so on the decomposition of it, when, to make the result unexceptionable, the oxygen it contained would either take the form of dephlogisticated air, or become a component part of some other substance into which oxygen is acknowledged to enter. But this has not been done. When it is decomposed by being fired together with dephlogisticated air, the fixed air which is the formed comes, I have no doubt, from the oxygen in the dephlogisticated air, and the phlogiston in this species of inflammable air; the same being the result, tho' not in the same degree, of firing the heavy inflammable air from charcoal and water, from oil &c. &c, into which it is not pretended that any oxygen enters.

It,

It, therefore, appears to me to be an absolute abandonment of the most fundamental principle of the new theory to call the air from finery cinder and charcoal an *oxide*. If substances be *combustible* in proportion to their affinity to oxygen, and their consequent readiness to unite with it, this air, which is inflammable, must be of *this* class, and therefore the very reverse of the *oxides*, which are saturated with oxygen, and incapable of receiving more.

5. If the oxygen, after quitting the finery cinder, entered into it again, it would make it finery cinder as at the first, or at least in some degree ; whereas the calx is completely revived in this process, the iron so revived being as soluble in acids as any iron whatever.

6. If the iron should be completely revived by the oxygen wholly leaving it, I maintain that it could not, by any degree of heat, decompose fixed air. For my experiment with a *burning lens*, in which it could not be done, is far more unexceptionable than Mr. Cruikshank's with *bladders and a gun barrel*. His objection to my process has no weight. It was made with only a very few ounce measures of the air, over mercury, with a lens sixteen inches in diameter, and was continued several hours, generally from ten o'clock to one ; so that no particle of the air could escape being exposed to a far greater degree of heat than could be communicated through a gun barrel.

His experiment I have frequently made, both in England and here, but I could never be satisfied with the result. The *scale* which he found upon the iron, I have no doubt, came from *moisture* in the air, or from  
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the bladders. Indeed, I cannot think that any person, conversant as I have been, with both these modes of operating, can hesitate in declaring that the preference must be given to mine.

7. Mr. Cruikshank seems to think that charcoal cannot contain any oxygen; but Mr. Lenant's fine experiment decisively proves that it does. For where are we to look for the oxygen (which we all acknowledge to be a component part of fixed air) which is separated from the marble, but in the charcoal which is produced? And in that it makes part of a *solid substance*, and does not take the form of *air*.

8. Since oxygen and all combustible substances unite, and explode together in a certain degree of heat, the oxygen that is expelled from the finery cinder, uniting with carbone from the charcoal when *red hot*, must enable it to burn, and therefore, in these circumstances, there ought to be an *explosion*, or at least a gradual *combustion*, of them in the course of the process, as there is when oxygen is put to the same substance, and heated with it afterwards. Here, however, oxygen meets carbone, a combustible substance, in a red heat, without any explosion or combustion taking place.

Thus do the antiphlogistians boast of the discovery of \* a *new oxide*, when they are unable to prove that it contains a particle of oxygen, and when its obvious properties shew that it belongs to a class of substances the

\* Mr Chenevix, a chemist of the first class in the new school, speaking of Mr. Cruikshank, calls him very justly "an able chemist, to whom we are indebted for the discovery of the gaseous oxide of carbone." See *Philosophical Transactions* for A. D. 1802, p. 133.

the very reverse of oxides. If the *discovery*, as it is called, relates to the *substance*, it belongs to me. All that they can pretend to is having given a better account of the *nature* of it, and with what success they have done this let the impartial reader judge.

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## SECTION V.

### *Of the Calces of Zinc.*

THE only circumstance that gives any plausibility to the opinion of finery cinder being an oxide of iron is the addition that is made to the weight of the iron when it is converted into this calx. But when zinc is treated in the same manner, steam being sent over it in a red heat, tho' inflammable air is procured, the zinc gains no addition of weight; so that in this case there is no pretence whatever for saying that the water is decomposed.

The substance that is produced in these circumstances I have somewhere called *flowers of zinc* because it is a calx of zinc; and at that time I presumed that it must have all the properties of the common flowers of

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zinc,



zinc, and contain oxygen. But I have treated this peculiar calx of zinc, made without access of air, in all the methods that I can think of, without being able to find any appearance of oxygen in it, any more than in finery cinder. When I heated it in common air, the air was not increased but diminished, the very same effect that is produced by the finery cinder.

Having put an ounce of zinc into a glazed earthen tube, to which I gave a red heat, I made steam pass over it till I had procured three hundred ounce measures of inflammable air, after which I found the greatest part of the zinc reduced to a dark coloured semitransparent glass, adhering pretty closely to the tube. I was able, however to separate them, and I am confident that the calx did not weigh more than the metal had done; whereas, computing from the proportion of eighty five parts of oxygen to fifteen of hydrogen (into which it is said that water is resolvable) it ought to have gained about a hundred grains. Since, then, this great proportion of oxygen is not found either in the calx, or in the water (for this I also examined) where will the antiphlogistians say that we are to look for it? For since the water, they say, is decomposed, in order to furnish the inflammable air, it ought to be found somewhere.

Another experiment that I made with zinc proves that when inflammable air is procured by means of it, it must come from the metal, and not from any water.

On throwing the focus of a burning lens on a quantity of zinc in common air, confined by water, in a glass vessel, the first effect is the production of *flowers of zinc*,  
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which make a beautiful appearance, by their dispersion within the vessel; and during this part of the process the air is diminished, the pure part of it, no doubt, entering the calx, while the phlogisticated part remains unaffected. After this, the application of the heat being continued, there is an increase of the quantity of air by the production of inflammable air; and instead of flowers of zinc, a *black powder* arises, and adheres to the inside of the vessel, and with care may be collected.

Now, since inflammable air is produced, the antiphlogistians must say, that part of the water over which the experiment was made, was decomposed. But then I ask, where is the oxygen, which, according to them, constitutes the far greater part of the water? I cannot find it any where. The *water* is entirely free from acidity, and the air expelled from it afterwards is even less pure than that which it yields before the process. And if I examine the *black powder* (which must be the metal sublimed) by heating it in confined common air, it becomes a whitish substance, the air is diminished, and rendered in a considerable degree impure; whereas, if it had contained any oxygen, the quantity would have been increased, and it would have been purer than common air; as when *red precipitate*, or *minium*, is treated in the same manner. It is evident therefore, that it contained no oxygen, but a quantity of phlogiston, on the expulsion of which, and the imbibing of pure air, it became flowers of zinc.

This experiment is rather more decisive than the similar one with iron, because the black powder to which

zinc is reduced can be affected by heat in common air, which finery cinder cannot.

It will hardly be pretended that the oxygen arising from the decomposition of the water is lodged in the flowers of zinc, since they were completely formed before any inflammable air was procured. Besides it will appear that little or no oxygen can be found in flowers of zinc produced in any process.

As I could not find any oxygen in the precipitates of iron dissolved in acids, I have not been able to find any in those of zinc. The most unexceptionable that I could think of was that by caustic volatile alkali. This substance I heated in atmospherical air, both moist and dry (lest exposure to the atmosphere should have made some difference in it) but it was with the same result. The air in which it was heated was made more impure than it was before, tho' in one case the quantity was increased from six and an half to eight ounce measures. Of this half an ounce measure was fixed air, and the remainder of the standard 1. 8. extinguishing a candle, so that it was almost wholly phlogisticated. It seemed, therefore, to have imbibed part of the pure air, and to have given out phlogisticated air.

Filings of zinc yield much inflammable air in pure water, tho' I do not find that they can by this means be reduced to a complete calx.\* But the imperfect calx to which the metal is then reduced, does not appear to contain any oxygen. When it was heated in atmospherical

\* Since this was first printed in the *Medical Repository*, I find that, by long standing, the surface of the filings of zinc is become white, so that they are perfect flowers of zinc.

spherical air the quantity of the air was increased, and about one twentieth part of it was fixed air, and the remainder was of the standard of 1. 5. The water in which the filings of zinc had been immersed, gave out air much worse than common air, and it was perfectly free from acidity. Iron filings will also yield inflammable air in water, and this water also gives out air that is more impure than common air, as does the water over which tin and other metals are calcined.

That the calces of metals do in general contain oxygen I have no doubt, because the dephlogisticated air in the atmosphere disappears when they are calcined in it. But there is reason to think that the greatest part of the addition of weight which they by this means acquire is from *water*, while the oxygen attaches itself to other substances in preference to the calx, if they be present.

One instance of this is that when they are calcined with a burning lens over lime water, the lime is precipitated; whereas if the calx had imbibed all the dephlogisticated air that disappeared, the lime water would not have been affected in the process; this precipitation of the lime, coming, no doubt, from fixed air, which I have sufficiently proved to consist of dephlogisticated air and phlogiston, or the base of inflammable air. I had this result when I calcined iron, copper, zinc, tin, lead, bismuth, and regulus of antimony in these circumstances. But when the process was made over mercury, I could not always find any fixed air; and therefore I presume that all the oxygen was imbibed by the calx, tho' it may be impossible in many cases to extract it again in that

form. For when the quantity is small, it may be so united to the phlogiston remaining in the calx, as to form the basis of phlogisticated air, which I have proved to consist of dephlogisticated and inflammable air.

Lead furnishes an example of this. No oxygen I believe can by any means be got from *massicot*, tho' it has imbibed some. But when this calx is supersaturated with it, and is become *minium*, it will yield the purest dephlogisticated air by heat only, and will likewise dephlogistate marine acid. And since flowers of zinc will not dephlogistate marine acid, I presume that this calx also is nearly in the same state with *massicot* in this respect; and that in any state it contains but little oxygen, or so united to phlogiston as not to be extracted either in the form of acid, or of dephlogisticated air.

Tho' the flowers of zinc may contain some oxygen, I have not been able to discover any in them by any process that I have made use of for the purpose. As this substance is formed in a considerable degree of heat, I was not surprized to find that heat would not expel any thing from it; but I thought that when it was mixed with iron filings it might with them, yield some fixed air, as red precipitate does. But I did not find this to be the case. I got nothing in this process besides inflammable air. Also, when mixed with perfect charcoal, such as gives no air with heat, a great quantity of both fixed and inflammable air is produced; which shews that, like this substance, flowers of zinc contain little or nothing besides water, which will have just the same effect.



To make this experiment with fairness, the iron filings must be heated till they give no air. They must then be well washed, till the water put on them be quite clear, and be again found to give no fixed air with heat. For foreign substances are very apt to be mixed with iron filings, and this process will separate them. With iron filings thus prepared, red precipitate gives fixed air, but flowers of zinc none.

There is a grey calx of zinc similar to that of lead, which Mr. Chaptal calls a *perfect oxyd*. This I find to be only zinc partially calcined. For on heating it in atmospherical air it became white, the air was diminished, was without fixed air, and considerably phlogisticated. The perfect flowers of zinc treated in the same manner made no sensible change in the quantity of the air; but, as in the former case, there was no fixed air in it, and it was considerably phlogisticated.

The melting of massicot in these circumstances made no change of any kind in the air, which shews that it contains no more phlogiston than flowers of zinc.

Oxygen in a calx is perhaps most easily detected by its forming fixed air when it is heated in inflammable air; but I did not find this to be the result of an attempt to revive flowers of zinc in those circumstances. Owing to the whiteness of this substance, which disposes it to reflect, and not to absorb, the light that is thrown upon it, I could not revive any part of this calx completely. A black spot only was made on a part of it, and about an ounce measure of inflammable air was imbibed; but I found no fixed air in the remainder, any more than I did when I revived finery cinder in the same process.

SECTION

## SECTION VI.

*Arguments in Favour of the Doctrine of Phlogiston from some Circumstances in which Sulphur is formed, and nitrous Acid phlogisticated.*

1. **A**N argument may, I think, be drawn in favour of the doctrine of phlogiston from my experiment of the formation of sulphur from the acid of vitriol heated in inflammable air, and also from water impregnated with vitriolic acid air, exposed in a close vessel to a continued heat.

Sulphur, the antiphlogistians say is a simple substance, and that the vitriolic acid is that substance with the addition of oxygen, or dephlogisticated air. Why, then, I ask, is not sulphur produced when dephlogisticated air is expelled from it by heat, rather than in the process with water impregnated with vitriolic acid air? For when this air is procured by making the acid pass through a red hot earthen tube, no sulphur is found. But when it is heated to dryness in inflammable air, which can supply it with phlogiston, sulphur is formed.

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The production of *phosphorus* from the phosphoric acid heated in inflammable air furnishes the same proof of this substance also being a compound, and that phlogiston enters into the composition of it, as well as into sulphur.

According to the phlogistic theory, the formation of sulphur from water impregnated with vitriolic acid air is very easy; both the ingredients of which it is composed being present, viz. its basis, vitriolic acid, and phlogiston. They are only made to form a different mode of combination by the heat in a tube hermetically sealed. For the vitriolic acid air is produced by heating in vitriolic acid most of the metals, or any other substance, solid or liquid, that is said to contain phlogiston.

If it be said that the sulphur may be formed in this experiment by the heat of separating the acid from its base; I answer that then the remaining water should be more acid than before; whereas I find it to be less so. This diminution of acidity I account for from the extreme volatility of this phlogisticated acid. But had the acid been that of vitriol unphlogisticated, it would have been obstinately retained by the water. Besides, it would, surely, be more easy to expel all acid from a liquor passing through a red hot open tube, than from a liquor confined in a glass tube, hermetically sealed, so that it cannot possibly escape; and when it is exposed to no more than a moderate degree of heat. For had it approached to a red heat, the tube would have burst.

But the formation of sulphur and phosphorus, by  
C 5 heating

heating the vitriolic and phosphoric acids, so as to evaporate them to dryness, in inflammable air, which then disappears, and this effect not being produced without it, or some other substance containing phlogiston, is, I think, decisive in favour of their receiving an addition of something from the inflammable air, or phlogiston, when they are converted into sulphur and phosphorus; and therefore that these substances are the compounds, and the acids the more simple substances of the two.

2. It is said by the antiphlogistians that the nitrous acid never becomes coloured by imbibing any thing, but always in consequence of giving out oxygen. I think, however, that the contrary is proved by its absorbing nitrous air, which it does with great rapidity. But the same effect is produced, tho' not in so remarkable a manner, by means of inflammable air.

I put a quantity of dephlogisticated nitrous acid into a phial with a ground glass stopper, with inflammable air on its surface; and in another similar phial atmospheric air was confined with it. Both these phials I covered with water in inverted glass jars, to prevent their having any communication with the atmosphere. After long exposure in these circumstances, that which had the common air on its surface never acquired any colour, or only a very little, from the effect of *light* transmitted through two glasses with water between them; but that on the surface of which inflammable air was incumbent acquired colour very soon. I also found, on repeating the experiment, that a part of  
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the inflammable air had been imbibed by the acid. In order to make this experiment, a phial filled with the acid must be introduced into a jar of inflammable air; and, part of it being poured out, the stopper must be put into it in that situation. Other precautions must be used which a little experience will teach.

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## SECTION VII.

### *Of the Calces of Mercury.*

**T**HE phlogistic theory, I readily acknowledge is most pressed by the phenomena of the calces of mercury. But in forming any general theory we must content ourselves with the fewest difficulties. It will hardly be pretended by the greatest admirers of the antiphlogistic theory, that it is attended with none. Those which attend the phlogistic with respect to these calces I do not think to be insuperable, and farther experiments may throw more light upon them.

As there are calces of mercury which certainly imbibe inflammable air, this substance, or the base of it, phlogiston

phlogiston, must be concluded to exist in that metal as an element. This is true both with respect to red precipitate, and turbith mineral.

As to the calx of mercury from the acid of vitriol, Mr. Beaume\* I find; agrees with me in the observation, tho' I did not know it at the time, that it is not completely reduceable by mere heat. But "later observations," Dr Maclean says, p 11, " shews that the turbith mineral, or any other substance into which it may be converted by a red heat, does not require any addition to constitute it a metal;" and Mr. Adet says, p. 43, " that the yellow oxide of mercury has been revived without addition by Messrs. Monnet, Bouquet, Lavoisier, and Fourcroy."

To this I can only say, that I have never been able to reduce the whole of this calx by any heat that I could apply, not even that of a burning lens of sixteen inches diameter, and this, I am confident, is a greater heat than can be raised by any furnace whatever. From being a red friable substance, this heat converts it into a yellowish glass, with the loss of about three tenths of its weight; but after this, no continuance of the same heat makes any farther change in it. Yet after this, when it is heated in inflammable air, the air is imbibed, and it is covered with a black powder evidently *ethiops mineral*, into which mercury, with all its component parts,

\* With Mr. Beaume I was a little acquainted. Mr. Macquer introduced me to him in his laboratory in Paris, and though he was an avowed opponent of the whole of the pneumatic chemistry, he was a good operator in the old way; and his fires, I am persuaded, were as hot as any raised by the persons mentioned by Mr. Adet, or those by Dr. Hope.

parts, whatever they be, is known to enter. This substance also, and not directly running mercury, was frequently the result of my experiments on this precipitate before I left England. This is certainly an experiment of considerable consequence. For if it be true that inflammable air be really imbibed by *any* calx of mercury, that it is revived by it, and can not be revived without it, we are authorised to say universally, that some element of which it consists, and no doubt phlogiston, is a necessary component part of that metal, and therefore of all the other metals also.

A decisive evidence, as it appears to me, that mercury contains phlogiston is the absorption of a great proportion of inflammable air in the revival of red precipitate in it.

By means of a burning lens I heated a quantity of red precipitate in inflammable air, in a glass vessel confined by water, till one hundred and twenty one ounce measures of the air were reduced to ninety five. Then, examining the residuum, I found that one measure of it mixed with an equal quantity of nitrous air occupied the space of 1. 77 measures. Computing from this result, it will be found that it contained 7. 22 ounce measures of pure air which added to the twenty six which had disappeared make 33. 22 ounce measures of inflammable air which had been absorbed by the calx in its revival. For that the air expelled from the calx had not contributed to the formation of *water*, was evident from its being found mixed with the remainder of inflammable air. Neither had it, in this case, contributed to the formation of fixed

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ed air. For there was not a great quantity of this air found in it, tho' I have formerly found a little of it in this process. Nor can this difference in the result be thought extraordinary, when it is considered that fixed air certainly consists of pure air and inflammable air, and that it is found in other processes similar to this.

In another experiment of this kind I revived a quantity of the precipitate in thirty ounce measures of inflammable air, till twelve ounce measures disappeared, and the standard of the remainder, examined as in the preceding case, was 1. 495. From this it appeared that 1. 495 ounce measures of air had been expelled from the calx, and that 12. 000 ounce measures of inflammable air had been absorbed by it.

Since much of the calx was sublimed in the process, the best method of determining how much inflammable air is imbibed is the revival of a given quantity of mercury, is to compare the quantity of pure air that is yielded by a given quantity of the calx with the quantity of inflammable air that corresponds to it in these experiments. Now an ounce of the precipitate yields about sixty ounce measures of pure air; and since in these experiments 46. 71 ounce measures of inflammable air were absorbed when 8. 71 ounce measures of pure air were emitted, sixty ounce measures could not be expelled without the absorption of three hundred and twenty three ounce measures of inflammable air; and since mercury gains, as Mr. Chaptal says, about 8 per cent. in being converted into precipitate, an ounce of mercury must contain three hundred and



and sixty two ounce measures of inflammable air, or rather the phlogiston that enters into it. An ounce of lead, I have shewn, requires one hundred and eight ounce measures of inflammable air, an ounce of bismuth one hundred and eighty five, of tin three hundred and seventy seven, of copper from verditer, four hundred and three, and of iron eight hundred and ninety.

That mercury revived either by inflammable air, or in close vessels, has the same properties will not be denied ; and if so, it must consist of the same principles, and in the same proportions, or nearly so. I am therefore inclined to think, improbable as it may appear, that the same principle which is essential to the constitution of inflammable air, that is phlogiston, passes from the fuel through the hot glass when the calx is revived by heat in a glass vessel.

There is however, only the choice of this difficulty, and of that of an ounce of mercury containing either three hundred and sixty two ounce measures of inflammable air (that is the phlogiston in it) or none at all. It is not denied that *light* and *heat*, both of which are allowed to be *substances*, tho' the weight of them cannot be ascertained, pass through glass. They both have certain properties, and are transferable from one substance to another, according to their known affinities. And why may not this be the case with *phlogiston* also ? The *electric matter* passes readily through glass when it is very hot, tho' it will not when it is cold.

Light certainly passes through glass ; and is known to give to some substances colour, smell, and taste,  
which

which have usually been ascribed to phlogiston. That it does not revive the lead in passing through the hot flint glass, is no sufficient objection. For the same substances in different combinations, and in different states, have different properties. The doctrine of chemical affinities has yet many difficulties attending it, and it requires the nicest discrimination of circumstances to make consistent tables of them. However, I can only propose *facts*, let others account for them in the best manner that they can. Mr. Scheele supposed that even dephlogisticated air, or the essential element of it, passed through glass.

I have frequently repeated this experiment of the revival of red precipitate in inflammable air, and have never failed to find a great absorption of it, whether there was any fixed air in the remainder or not; and I should have repeated it much oftener, and on a larger scale, in order to ascertain with more exactness the quantity of inflammable air, or of phlogiston, contained in a given quantity of mercury, but that it has frequently happened that the vessels in which I made the experiments were exploded, after a sufficient quantity of pure air was expelled from the calx. This accident, however, is a proof that the air expelled from the precipitate had not formed either water or fixed air. Sometimes, however, I have made the greatest part of the inflammable air to disappear without any explosion.

The accuracy of this experiment being questioned by Dr. Woodhouse, I repeated it with all the attention I could give to it, and had the following result. I heated

ed a quantity of red precipitate in twenty eight and an half ounce measures of inflammable air, till it was reduced to twenty four and an half, and found that, whereas before the process it was not in the least affected by nitrous air; the standard of it was afterwards 1. 8; so that it contained a considerable mixture of dephlogisticated air. I repeated the experiment several times, and always found pure air mixed with the inflammable, when I had revived any part of the calx.

Continuing one of these processes, till, after the diminution, the quantity of air began to increase, there was an explosion; but it only raised the receiver in which the air was confined about an inch, and recovering its position, it broke the earthen dish in which it was placed.

After this I made use of a tin dish, and repeating the experiment, there was an explosion so loud, that a person at a considerable distance was alarmed, and came running to see what had happened. The receiver, which was a very heavy one, was blown much higher than my head; but falling on the grass, it was not broken. After this, I thought it unnecessary to make any more experiments of the kind.

Having formerly made many experiments on the revival of red precipitate in inflammable air, when I was a convert to the doctrine of the composition of water, I shall subjoin what I then observed with respect to the subject from the 6th volume of my *Observations on air*, p. 128.

“The greatest difficulty that occurred with respect to the theory of the constitution of water, arose from

“ my never having been able to procure any *water* when  
“ I revived mercury from red precipitate in inflammable  
“ air, or at least more than may be supposed to have  
“ been contained in the inflammable air. In order to  
“ make the experiment with the *scales of iron*, and that  
“ with the *precipitate*, as much alike as possible, and  
“ that I might compare them to the greatest advantage,  
“ I made them immediately one after the other, and  
“ with every circumstance as nearly as I could the same.  
“ The inflammable air was the same in both experiments,  
“ and the scales of iron, and the precipitate, were made  
“ as dry as possible. They were heated in vessels of the same  
“ size and form, and equally confined by dry mercury. And yet  
“ when I heated the former, water was formed as copiously as I  
“ have described it before, viz. actually running down the inside  
“ of the vessel in drops, tho’ only four ounce measures of  
“ inflammable air were absorbed. But tho’ I heated the precipitate  
“ till eight ounce measures of the air was absorbed, and only three  
“ fourths of an ounce measure remained, there was hardly any  
“ sensible quantity of water produced, certainly not one tenth of what  
“ appeared in the experiment with the scales of iron. There was  
“ this difference, however, in the two results. In what remained  
“ from the experiment with the precipitate I at this time perceived  
“ a slight appearance of fixed air, whereas there was none in  
“ what remained in the scales of iron. The residuum also from  
“ the precipitate had in it a small portion of dephlogisticated  
“ air. For being mixed with an equal measure of nitrous air the  
“ standard of it was 1. 8. In  
“ this

“ this experiment there can be no doubt but that the  
“ dephlogisticated air dislodged from the precipitate  
“ mixed with the inflammable air in the vessel, and as  
“ no water was produced, they must have formed some  
“ more solid substance, which in the small quantity I was  
“ obliged to use could not be found.”

At this time, however, I think it more probable that nothing *solid* was produced, but only that the phlogiston of the inflammable air was imbibed by the calx, while the pure air emitted from it was in part found mixed with the inflammable air in the vessel, and in part united with it and formed fixed air.

In nine ounce measures of inflammable air from malleable iron and water I revived part of the precipitate sent me by Mr. Berthollet, (which I had found to contain no fixed air) till not more than one fourth of the air remained unabsorbed ; and examining it, I found one twentieth part of it fixed air. But mixing nitrous air with it, it appeared that the air dislodged from the precipitate had not wholly united with the inflammable air. For being mixed with an equal quantity of nitrous air it occupied the space of 1. 71. After the process I missed eighteen grains of the precipitate: But there are several causes of loss in this case, besides that from the air expelled from it.

In 5. 5 ounce measures of the same inflammable air I again revived some of the same precipitate till it was reduced to 0. 77 of an ounce measure. Of this one sixth part was fixed air, and the rest of the standard of 1. 6. It exploded at once when a flame of a candle was presented to it.



In making these experiments over mercury, we necessarily use but small quantities of air, and therefore the results may not in some respects be so much depended upon. But I think it sufficiently appears from them that no *water* was formed in the process, and this the new theory absolutely requires.

On the whole, I think it can hardly be denied that considering the great quantity of inflammable air that disappears in these experiments, the greatest part of it, at least, must enter into the calx. And since all running mercury must consist of the same elements, the same principle that (with the addition of water) forms inflammable air, and which we call phlogiston, must pass through the red hot glass when the calx of mercury is revived without addition, by means of heat only.

Some experiments that I have made on silver, gold, and platina, favour this hypothesis. All these metals yield a considerable quantity of nitrous air, when they are dissolved, the first in nitrous acid, and the last two in aqua regia. And when the solutions were evaporated, and the residuums heated in inflammable air, a great quantity of it disappeared, and the metals were revived. And yet by means of the same acids these dry residuums will yield a great quantity of nitrous air. They must, therefore, have acquired by means of heat only, and this transmitted through a vessel not red hot, the same principle that was communicated to them by imbibing inflammable air.

That nitrous air contains the same principle with inflammable air, or phlogiston, appears from the following



lowing experiment, in which the former was produced by means of the latter, if the nitrated calx of any metal be heated in it.

If copper be dissolved in nitrous acid, and the water be expelled to a certain point, there remains a *green substance*, which is not at all deliquescent; but when exposed to heat gives out a red vapour. Some of this substance I heated in twenty one ounce measures of inflammable air till the vessel was filled with red vapour, when it was reduced to six ounce measures, and I found that when it was mixed with common air, the standard was 1. 35; so that it was almost wholly nitrous air. There was in it a small quantity of fixed air, but there was nothing inflammable in it. It extinguished a candle.

I formerly endeavoured to ascertain the proportion of phlogiston in nitrous and inflammable air, and found it to be nearly the same in both. That this is not far from the truth, may, I think, appear from comparing the result of two of my former experiments, which I never before thought of doing with this view. When I first discovered nitrous air, I endeavoured to find what quantity of it would be yielded by the different metals, and found that twenty grains of iron yielded sixteen ounce measures. When, with other views, I endeavoured to ascertain the quantity of inflammable air that was yielded by malleable iron, I found that one hundred and twenty grains of it yielded ninety six ounce measures; and this is exactly the quantity of nitrous air that the same weight of iron would give. For 120 is to 96 as 20 is to 16.

Twenty grains of platina gave nine ounce measures of pure nitrous air, and twenty two grains of gold gave eight ounce measures. They therefore contain nearly the same proportion of phlogiston (for 20 is to 9 as 22 is to 9. 9) and little more than half as much as iron. For it will be in the proportion of one hundred and seventy ounce measures to the ounce. It is, however, more than is contained in lead, but less than in bismuth, and much less than in mercury.

That something can pass through glass is evident from many observations respecting both light and heat, one of the most remarkable of which is perhaps that of minium, or red precipitate (which when cold are of the colour of arterial blood) heated in a glass tube, acquiring the dark colour of venous blood, tho' they lose it again when they become cold. What to infer from this curious fact I do not distinctly see.

## SECTION

## SECTION VIII.

*Experiments on heating Manganese in inflammable Air.*

SOME experiments that I made on heating manganese in inflammable air authorize the same general conclusion with those on heating red precipitate in that kind of air, and therefore deserve to be recited.

Manganese and red precipitate of mercury are substances of a similar nature, as they are both calces of metals, and both contain dephlogisticated air, and, like all other calces of metals, they imbibe the inflammable air in which they are heated. But whereas the dephlogisticated air expelled from the precipitate, frequently at least, remains mixed with the inflammable air (which is sometimes the cause of dangerous explosions) this has never happened to me in consequence of heating manganese in the same circumstances, so that it may be operated upon without any apprehension.

'Tho' in these experiments the inflammable air disappears, and dephlogisticated air is expelled from both the substances, no *water* is formed by means of either of them, as the new theory absolutely requires, and which is pretended to be the case when finery cin-  
der is heated in the same circumstances. But since the

same quantity of inflamed air disappears in all these cases alike, the same quantity of water, if inflammable air be readily a constituent part of it, ought to be produced. It is evident, therefore, that the water produced by means of fiery cinder was actually contained in that substance, and only expelled by the heat, to make room for the phlogiston which then enters into it, by the absorption of the inflammable air; while the precipitate and manganese, containing no water, imbibe the inflammable air without any appearance of water, except the little that is the basis of the inflammable air, or may be concealed in such powdery substances as manganese and the precipitate are.

The principal difference, however, in the result of these two similar processes is that in the experiment with the manganese a quantity of *phlogisticated air* appears to be generated, which is not the case with the precipitate. The particulars of the experiments were as follows.

Fifteen ounce measures of inflammable air were reduced to seven by heating in it twenty four grains of manganese. In the residuum, which was not affected by nitrous air, there was no fixed air; but when a certain portion of it was fired together with a certain quantity of dephlogisticated air, the diminution was only to 0. 95; whereas when the same quantity of the same original inflammable air was exploded in the same circumstances, the diminution was to 0. 52. In another similar experiment, in which the inflammable air was confined by mercury, and in which seven ounce measures were reduced to one and a quarter, the diminution

with

with the residuum was to O. 93, when that with the original inflammable air was to O. 33. The manganese I used in these experiments yielded dephlogisticated air without any mixture of fixed air.

I repeated this experiment with manganese out of which air had been expelled with a strong heat, and the result was the same as with the fresh manganese, except that much more heat was necessary to make it imbibe the air; whereas the manganese from which the dephlogisticated air has been expelled imbibes inflammable air very rapidly, and with a very moderate degree of heat. Twenty grains of it imbibed eight ounce measures of inflammable air, and then weighed nineteen grains. In both the kinds of manganese the central part on which the focus of the lens fell was of a beautiful green colour, but the next day this had disappeared, and the whole of the substance that had been heated was brown.

As no doubt dephlogisticated air is expelled from the manganese in these experiments, and neither fixed air nor water is produced, the dephlogisticated air must enter into the fixed air that is found in the residuum.

In order to make a more exact comparison between the experiments in which manganese and finery cinder were made to imbibe inflammable air, I repeated them both in similar vessels, with equal quantities of inflammable air confined by mercury; and I observed that, tho' some water appeared during the process with the manganese, it wholly disappeared the next morning; whereas the inside of the vessel in which the finery cinder had been revived was wholly covered with innumerable drops of water, contiguous to one another.

SECTION

## SECTION IX.

*Of the Decomposition of Water.*

THE antiphlogistic theory has received its greatest support from the supposed discovery that water is resolvable into two principles, one that of *oxygen*, the base of dephlogisticated air, and the other (because it has no other origin than water) *hydrogen*, or that which with the addition of *calorique*, or the element of *heat*, constitutes inflammable air. “One of the parts of the modern doctrine the most solidly established, say Mr. Berthollet, and the other authors of the *Report* on this subject (*Examination of Kirwan*, p. 17) is the formation, the decomposition, and recomposition, of water. And how can we doubt of it, when we see that, in burning together fifteen grains of inflammable air, and eighty-five of vital air, we obtain exactly an hundred grains of water, in which, by decomposition, we find again the same principles, and in the same proportions. If we doubt of a truth established by experiments so simple, and palpable, there would be nothing certain in natural philosophy. We might even



“ even question whether vitriolated tartar be compos-  
 “ ed of vitriolic acid and fixed alkali, or sal ammoniac  
 “ of the marine acid and volatile alkali, &c. &c. For the  
 “ proofs that we have of the composition of these salts  
 “ are of the same kind, and not more rigorous than  
 “ those which establish the composition of water. Noth-  
 “ ing perhaps more clearly proves the weakness of the  
 “ old theory, than the forced explanations that have  
 “ been attempted to be given of these experiments.”

Notwithstanding the confidence thus strongly expressed by these able and experienced chemists, I must take the liberty to say, that the experiments to which they allude appear to me to be very liable to exception, and that the doctrine of phlogiston easily accounts for all that they observed.

Their proof that water is decomposed, and resolved into two kinds of air, is that when steam is made to pass over red-hot iron inflammable air is produced, and the iron acquires an addition of weight, becoming what is called *finery cinder*, but what they call an *oxide of iron*; supposing that there is lodged in it the oxygen which was one of the constituent parts of the water expended in the process, while the other part, or the hydrogen, with the addition of heat, assumed the form of inflammable air.

But in order to prove that this addition of weight to the iron is really oxygen, they ought to be able to exhibit it in the form of dephlogisticated air, or of some other substance into which oxygen is allowed to enter, and this they have not done. Iron that has really imbibed air, or the common *rust of iron*, has a very different appearance

appearance from the finery cinder, being *red*, and not *black*; and when treated in similar processes, exhibits very different results. Mr. Fourcroy says, (*Ibid* p. 251) that this finery cinder is "iron partially oxygenated." But if that were the case, it would go on to attract more oxygen, and in time become a proper rust of iron completely oxygenated; whereas this is so far from being the case, that as I have observed finery cinder never will acquire rust; which shews that the iron in this state is saturated with some very different principle, which even excludes that which would have converted it into rust.

However, neither this, nor any other calx of iron, can be revived unless it be heated in inflammable air, which it eagerly imbibes, or in contact with some other substance which has been supposed to contain phlogiston. The probability therefore is, that the phlogiston then enters this calx of iron, replacing that which had been expelled to form the inflammable air. Nor can any inflammable air be procured in this process with steam, but by means of some substance which has been supposed to contain phlogiston. Where then, is the certain proof that water is decomposed in this process?

Since, according to the antiphlogistic theory, water itself contains all the elements of both dephlogisticated and inflammable air, and wants only *calorique*, which they can give at pleasure, I see no reason why heat alone, without the aid of any metal, might not convert it into air. When the particles are so far separated as they are in a state of steam, I see no occasion for the superior

perior attraction of any other substance for either of them. In steam each of the elements is already in the form of air, and with its due proportion of calorique, and then why should they not continue in that form, only mixed together, ready for explosion?

It is said that the oxygen imbibed by this iron, being expelled by heat in contact with inflammable air, unites with that air, and with it constitutes the water which is found after the process. But for any thing that appears, this water may be that which the iron had imbibed, and which can only be expelled from it by the entrance of that phlogiston which it had lost. Besides, it has been shewn that the water produced in this manner is much more than in the required proportion of the inflammable air that disappears.

Another pretended proof that water is composed of dephlogisticated and inflammable air, is that when the latter is burned slowly in the former, they both disappear, and a quantity of water is produced equal to their weight. I do not, however, find, that it was in more than a single experiment that the water so produced is said to have been entirely free from acidity, tho' this experiment was on a large scale, not less than twelve ounces of water being procured. But the apparatus employed does not appear to me admit of so much accuracy as the conclusion requires; and there is too much of correction, allowance, and computation in deducing the result.

And so, it is, after all, acknowledged that, after decomposing this quantity of the two kinds of air, and making all the allowance they could for phlogisticated  
air

air, or *azote*, in the dephlogisticated air, the experimenters found fifty one cubic inches of this kind of air more than they could well account for. This quantity, therefore, and perhaps something more (since the operators were interested to make it as small as possible) were formed in the process. And when this kind of air, as well as inflammable is decomposed together with dephlogisticated air, nitrous acid is produced. The probability therefore is, that the acidifying principle, or the oxygen, in the dephlogisticated air which they decomposed, was contained in that phlogisticated air, and that, had the process been conducted in any other manner, it would have assumed the form of nitrous acid. They acknowledge that, except when the inflammable air was burned *in the slowest manner*, the water they produced had more or less of acidity.

The reason, no doubt, was that, whenever the flame they made use of was too strong, more of the dephlogisticated air in proportion to the inflammable was consumed than when the flame was weak ; so that the results of their experiments exactly coincide with those of mine.

Citizens Berthollet and Fourcroy say, with Mr. Adet, that " the small quantity of acid which is commonly " found in this process comes from the *azote*, which is " mixed with the gas." (*Annales de Chymie*, vol. 26, p. 306.) But if this was the case, they could never get water free from acidity, because they can never wholly exclude azote. Besides, how can they think it so easy to procure nitrous acid from azote in this process, when

when Mr. Cavendish found it so difficult to procure a barely sensible quantity by numberless electric explosions?

The experiments which I made on the decomposition of these two kinds of air in *close vessels*, appear to me to be much less liable to exception, and the conclusion drawn from them is the reverse of that of the French philosophers.

When dephlogisticated and inflammable air, in the proportion of a little more than one measure of the former or two of the latter (both so pure as to contain no sensible quantity of phlogisticated air) are inclosed in a glass or copper vessel, and decomposed by taking an electric spark it it, a highly phlogisticated nitrous acid is instantly produced; and the purer the airs are, the stronger is the acid found to be. If phlogisticated air be purposely introduced into this mixture of dephlogisticated and inflammable air, it is not affected by the process, though, when there is a considerable deficiency of inflammable air, the dephlogisticated air, for want of it, will unite with the phlogisticated air, and, as in Mr. Cavendish's experiment, form the same acid. But since both the kinds of air, viz. the inflammable and the phlogisticated, contribute to form the same acid, they must contain the same principle, viz. phlogiston.

If there be a redundancy of inflammable air in this process, no acid will be produced, as in the great experiment, of the French chemists, but in the place of it there will be a quantity of phlogisticated air together with water.\*

Messrs.

\* I found, after many trials, that when an hundred ounce measure



Messrs. Berthollet and Fourcroy say, with Mr. A. det, that the water procured in this manner cannot be held in solution in the gasses, but must necessarily be a new production (*Annales de Chymie*, vol. 26, p. 306) But I do not say that this water was held in solution in the gasses, but was a constituent part of them; and for any thing that is certainly known is all that can be ascertained by *weight*. I wish, however, to have more repetitions of this experiment, in order to ascertain this curious circumstance. I was never able to get the whole weight of the airs in water. In my experiments, when no acid is produced a considerable quantity of phlogisticated air is always formed.

When the decomposition of phlogisticated and inflammable air is made in a glass vessel, a peculiar *dense vapour* is formed, which the eye can easily distinguish not to be mere vapour of *water*, and if the juice of turnsole be put into the vessel, it immediately becomes of a deep red, which shews that it was an acid vapour.

Since the acid that I procured in this process was in considerable quantity, and no phlogisticated air was present (for in the last of the experiments I did not even make use of an air pump, but first filled the vessel with water, and then displaced it by the mixture of the airs) I do not see how it is possible to account for the formation of this acid but from the union of the two kinds of air; and it can hardly be supposed that, in the very same process, the decomposition of the same substances should

inflammable air were fired together with fifty one measures of dephlogisticated air *acid* was formed, but that only *water* was produced when the same quantity of inflammable air was fired with forty seven measures of the same dephlogisticated air.



should compose others so very different from each other as *water* and *spirit of nitre*. I think I have sufficiently accounted for the result of the experiments made by the French chemists on the common hypothesis, which supposes inflammable air to contain phlogiston; but I do not yet see how it is possible for them to explain mine on theirs; according to which there is no such principle in nature. Upon the whole, it does not appear to me that the evidence either for the composition, or the decomposition of water, is at all satisfactory; and certainly the arguments in support of an hypothesis so extraordinary, and so novel, ought to be of the most conclusive kind.

Dr. Maclean boasts greatly of the superior accuracy of the French chemists. "In what respects," says he, p. 45, "his experiments (meaning mine) were less liable to exception than those of the French chemists, is what I do not comprehend. Theirs were performed on a very extensive scale, great care was taken to ascertain the degree of purity of the gasses before combustion, and the apparatus was so constructed, that the results could be determined with the greatest nicety. The Doctor's, on the contrary, were made with very trifling quantities of materials, their purity was not tried, and their weight not accurately determined."

Let us now consider what these high sounding words amount to. Experiments made with a great quantity of materials are not, always, on that account, the most accurate, especially where, as in this case, the thing to be determined is simply the *quality* of the result. When I can produce but a few drops of a strong *acid*, and as

often as I please, from the very same materials from which I am told I ought to get only *pure water*, what is it to me whether they produce gallons?

Great care, he says, was taken to ascertain the purity of the gasses, whereas, with respect to mine, he says, the purity was not tried. Now that of mine was not only tried, and with as great accuracy as they could try theirs, but the dephlogisticated air that I used was purer than any that I believe they ever pretended to have made. For with two equal measures of nitrous air, the residuum was only four hundredth parts of a measure, and this slight impurity was probably not in the dephlogisticated, but in the nitrous air, which is very apt to vary in its quality, and very difficult to obtain pure. And yet with this very pure dephlogisticated air, and a proportion, exactly defined, of the purest possible inflammable air, I got drops of a stronger acid than can be procured by means of air less pure. To this impurity, viz, a mixture of phlogisticated air, the anti-phlogistians always ascribe the production of the acid, tho' if the air be purposely less pure, I never fail to find that impurity, viz. the phlogisticated air, unaffected by the process; so that it could not possibly have contributed to the production of the acid.

With the greatest confidence, however, Dr. Maclean says p. 53, "the dense acid vapour that I produced by the explosion of the two kinds of air was occasioned by the azote contained in the oxygenous gas that I employed." He might as well have said it was occasioned by that which I did *not* employ. If ten times the quantity of azote in the air I used had been wholly decom-

decomposed, it would not have amounted to the hundredth part of the weight of the acid I procured

Their apparatus, he says, was so constructed, that the result could be determined with the greatest nicety. On the contrary, it was extremely complex, as a view of their plates will shew, and mine was perfectly simple, so that nothing can be imagined to be less liable to be a source of error. How, indeed, was this possible? I use only one large vessel of glass or copper. I put into it at once a certain proportion of the two kinds of air, the purity of which, when it is necessary, I can ascertain as well as other persons. From the simplicity of the apparatus no other substance can possibly mix with them, and I then explode the whole at once by an electric spark. After this I presently find the result by examining the liquor that is drained from the vessel. Tho' I have not gallons of this liquor, I have some ounces, which no antiphlogistian would care to drink. Will Dr. Maclean say that my process is less accurate than that of the French, because it can be finished in less than five minutes, and theirs requires the assiduous attendance of some days?

Using the same most simple apparatus, I can, by only varying the proportions of the two kinds of air, produce the result which the French chemists so much boast of. For I can procure water as free from acidity as theirs, and with much greater certainty, as I have no attention to give to a flame, lest it should at any time burn too fiercely. But in this case I always produce a quantity of *phlogisticated air*, in which they acknow-

ledge that the principle of acidity resides. They also do not deny that they had a surplus of the same kind of air ; and as to the *quantity* of it, I cannot help supposing, that, interested as they were to make it as little as possible, being *men*, and of course liable to the biases of other men, they may have represented it, by the allowances they made in their computation, something less than it really was. All the inside of my large vessel being, of course, wet with the liquor produced by the explosion, I could not pretend to *weigh* that which was drained from it with much accuracy. But then very little depended upon the *quantity*, compared to the consideration of the *quality* of the liquor ; which may be as satisfactorily ascertained by drops, as by the largest quantities ; and till the French chemists can make their experiments in a manner less operose and expensive, requiring fewer precautions, and less of computation, I shall continue to think my results more to be depended upon than theirs.

Mr. Berthollet objects to my experiment, that the weight of the liquor which I produced from the decomposition of the dephlogisticated and inflammable air was never equal to the weight of the air. But this I account for by the escape of the highly phlogisticated acid.

He also says that I took no account of the residuum of the air in the vessel in which I made the explosions. But I did not overlook this circumstance ; since I measured the capacity of the vessel by the quantity of air that actually disappeared, by having been completely decom-

decomposed in the process ; so that there was no occasion whatever to take an account of the air that was not affected by it.

I shall conclude this section with observing that in order to complete their proof of the decomposition of water, the antiphlogistians should produce some substance which by uniting with hydrogen in water, should let go the oxygen, in the form of dephlogisticated air, or some acid ; and surely some such substance may be found, if their theory be true. The component parts of water are not so intimately united, but that they may be separated by means of affinities. Otherwise the hydrogen could not have been detached from it.



## SECTION X.

*An Argument against the Decomposition of Water from the different Proportions of the Elements of which it is supposed to consist, according to different Experiments.*

ACCORDING to the new theory, water consists of two principles, oxygen and hydrogen; and they are separated by iron, or charcoal, in a red heat, uniting with one of them, and suffering the other to escape; and therefore if, in any case, a quantity of water be wholly expended in forming air, and only one of the kinds be found, it will be evident that this water does not consist of two elements. Now according to one of my experiments water would appear to consist of only one of the kinds of air, and according to another of the other.

I have shewn that by a slow supply of water in sending steam over red hot charcoal, the whole of the produce is inflammable air, without any mixture of fixed air, or the production of any thing, aerial, fluid, or solid, into which oxygen can be supposed to enter. From this experiment, therefore, conducted in this manner, it



it might be concluded that water consists of hydrogen only, without any oxygen.

This observation of mine is confirmed by Mr. Watt, whose accuracy no person will call in question. He says in his *Description of apneumatical apparatus* subjoined to Dr. Beddoes's *Considerations on the medicinal use of factitious air*, p. 84. "It has been observed by Dr. Priestley, and confirmed by my experience, that "when much water passes in the form of steam, there "is much fixed air formed; but little or none when "the water is admitted so sparingly that no steam "reaches the refrigeratory.

I once thought with the Antiphlogistians that fixed air was held in solution in this heavy inflammable air, because when it is fired together with dephlogisticated air much fixed air is produced; but I am now satisfied that all the oxygen in that fixed air comes from the dephlogisticated air with which it is fired. That this *must* be the case in some experiments is evident, because the fixed air so procured greatly exceeds the weight of all the inflammable air employed; and therefore could not possibly have been contained in it.

The reason why more fixed air is produced when the supply of water is copious is, I imagine, because more water is necessary to the constitution of fixed air than to that of inflammable air.

According to this experiment with charcoal, water may be shewn to consist of hydrogen only; but according to my experiments with *terra ponderosa aerata*, it may be proved to consist of oxygen only. For when steam is sent over this substance in a red heat nothing

but the purest fixed air is produced; and yet the whole of any quantity of water may be expended in that production. As water is not said to contain any *carbone*, this must be supplied by the *terra ponderosa*, and all the oxygen by the water. For according to the theory fixed air consists of twenty eight parts of *carbone*, and seventy two of oxygen.

These experiments favour my hypothesis that water is the basis of all kinds of air, and therefore that without it no kind of air can be produced. In some cases, as in that of the light inflammable air, it may be all that can be ascertained by weight.

To my experiments with the *terra ponderosa*, which in my opinion demonstrably proves that water is a constituent part of fixed air, and therefore probably of other kinds of air also, Mr. Berthollet objects (*Report*, p. 82) that I did not examine the loss of weight in this substance. But after the process it adhered so closely to the earthen tube in which the experiment was made, that the loss of weight could not be ascertained with accuracy. This, however, was not at all necessary. I found very exactly how much fixed air a given quantity of this substance would yield by means of water, which appeared to be the same that it yielded by solution in marine acid, and that it yielded no air at all by mere heat without water. It was quite sufficient, therefore, to find how much water was expended in procuring any quantity of fixed air from this substance. And as there was no other source of loss of water besides the fixed air, it could not but be concluded that

that it entered into its composition, as a necessary part of it, and in the proportion which I ascertained.

The truly ingenious and equally candid Mr. Rupp, hints that the water might be imbibed by the terra ponderosa ; but I see no reason to think that it did. It is not at all probable, that there is any affinity between this substance and water ; and if water exist in it as an extraneous substance, the heat that I applied would have expelled it.

Mr. Rupp produces several experiments, made seemingly with great accuracy, to prove that fixed air contains no water. But experiments which require the solution of substances in acids, and evaporation, together with the computation of the proportion of earth, acid, and water, contained in salts, are much more complex than mine ; and therefore, will not, I think, authorize so positive a conclusion. I have not repeated his experiments, and leave others more expert than I am in such processes to judge between us.

## SECTION

## SECTION XI.

*Of the supposed Decomposition of Water in the Experiments of Van Troostwick and Deiman, in those of Mrs. Fulhame, and various other Processes.*

IT is alleged in favour of the decomposition of water, that both dephlogisticated and inflammable air have been procured by taking electric explosions in water. Experiments with this result were made by Messrs. Van Troostwick and Deiman, and have been repeated with the greatest attention by Dr. Pearson. See the *Philosophical Transactions* for 1797, p. 142.

The accuracy of these experiments I am by no means disposed to question. Both dephlogisticated and inflammable air, were, no doubt, produced, tho' with infinite labour, by this means; and I consider the experiment as exceedingly curious and important in several respects. But it is a very complex one. Several agents are concerned, and what, and how much, to ascribe to each of them is not easy to say. I have not yet found any termination to the production of air from water only, and the last product, which is equable, is wholly phlogisticated air, of the nature of which we know but little.

little. Some of my experiments seem to prove that it is composed of dephlogisticated and inflammable air; and *light*, which is peculiarly intense in the electric spark, is in my experiments on plants, and probably in other processes, a necessary agent in the production of dephlogisticated air, when there is water for its basis. And the metals that are employed, viz. gold and platina, may contribute to this slow production of inflammable air. But the accension of these airs being sometimes spontaneous, without the electric spark being taken in them, shews that part at least of the air produced is *phosphoric*; and it is well known that the electric spark is always accompanied with the smell of phosphorus.

I hope these experiments will be repeated with a still greater variety of circumstances, tho' I do not see how they can be made so that water only shall be employed, except perhaps in a glass syphon, so that the electric spark shall be made to pass from the water in one of the legs to the water in the other, and to this there are many objections.

To these observations I shall subjoin what I observed with respect to this experiment when I made a new arrangement of my *Observations on Air*, in three volumes, vol. 3, p. 543.

“ It must be acknowledged that substances possessed  
 “ of very different properties may be composed of the  
 “ same elements, in different proportions, and different  
 “ modes of combination. It cannot, therefore, be said  
 “ to be absolutely impossible but that water may be com-  
 “ posed of dephlogisticated and inflammable air, or of  
 “ any



“ any other elements. But then the supposition should  
 “ not be admitted without proof ; and if a former the-  
 “ ory will sufficiently account for all the *facts*, there is  
 “ no occasion to have recourse to a new one, attended  
 “ with no peculiar advantage.”

“ Also that phlogiston is an element, in the composi-  
 “ tion of water is, as I have more than once observed,  
 “ not at all improbable, since water conducts electricity  
 “ like metals and charcoal, into which the same princi-  
 “ ple enters ; and because, when fresh distilled, it at-  
 “ tracts dephlogisticated air from the atmosphere, which  
 “ is the property of other substances containing phlo-  
 “ giston. By this means water may, in fact, contain  
 “ both the principles of which, according to the new  
 “ theory, it wholly consists, and in what proportion it  
 “ contains them we cannot tell. For tho’ heat may ex-  
 “ pel them in part, in the form of air, the force of this  
 “ action may be limited, so that water boiled ever so  
 “ long may retain much air, which only such a degree  
 “ of heat as is communicated by electricity can disco-  
 “ ver. But this proves nothing against the doctrine of  
 “ phlogiston ; since it only proves that this principle is  
 “ contained in water, more or less intimately combined,  
 “ as well as in many other substances.”

Dr. G. Fordyce found, by an experiment which has the appearance of great accuracy (See *Philosophical Transactions* for 1792, p. 374) that the addition of weight to zinc, when it is converted into a calx, comes from the *water*. But he advances nothing to prove that the water was decomposed in the process ; and water is all that I can find in flowers of zinc.



It is pretended that water is decomposed by the growth of plants acted upon by light. But if this was the case, why will not a plant continue to grow in the same water till the whole of it be decomposed? Whereas I always found that only a certain quantity of dephlogisticated air could by this means be procured in the same water, and very little in proportion to its bulk. After this the production of air ceased, and the plant died.

To me it appeared, that the food of the plant was the phlogiston contained in the water. For when by the growth of the plant the air contained in the water was perfectly pure, the process always ceased. But the same plant, removed to water that contained impure air, would grow again, and give pure air as before. See my *Experiments on Air*, Vol. 5, p. 25.

Also, having put various vegetable and animal substances into water, which by putrefying became offensive in the dark (yielding inflammable air mixed with fixed air) and when the vessel containing them was placed in the light, and green vegetable matter was suffered to grow in it, the purest dephlogisticated air was produced; the phlogiston, as I observed p. 42, which, in other circumstances, would have been converted into inflammable air, now going to the nourishment of the plant, and by the influence of light yielding such pure air. On this subject I then made the following remarks, p. 62.

“ It is impossible not to observe from these experiments the admirable provision there is in nature to prevent, or to lessen, the fatal effects of putrefaction,  
“ especially

“ especially in countries where the rays of the sun are  
 “ most direct, and the heat the most intense. For where-  
 “ as animal and vegetable substances, by simply putrefy-  
 “ ing would necessarily taint great masses of air, and  
 “ render it wholly unfit for respiration, the same sub-  
 “ stances putrefying in water, supply a most abundant  
 “ pabulum for this wonderful vegetable substance, the  
 “ seeds of which appear to be in all places, dispersed  
 “ invisibly thro’ the atmosphere, and capable, at all sea-  
 “ sons of the year, of taking root, and immediately  
 “ propagating themselves to the greatest extent. By  
 “ this means, instead of the air being corrupted, a vast  
 “ addition of the purest air is continually thrown into  
 “ it.”

“ By this means also stagnated waters are rendered  
 “ much less offensive and unwholesome than they  
 “ would otherwise be. That froth which we see on  
 “ the surface of such waters, and which is apt to create  
 “ disgust, generally consists of the purest dephlogisticat-  
 “ ed air, supplied by aquatic plants which always grow  
 “ in the greatest abundance, and flourish most, in water  
 “ that abounds with putrid matter. When the sun  
 “ shines these plants may also be seen to emit great  
 “ quantities of pure air.”

“ Even where animal and vegetable substances pu-  
 “ trefy in air, as they have some moisture in them, va-  
 “ rious other plants, in the form of *mold*, &c. find a pro-  
 “ per nutriment in them ; and by converting a consider-  
 “ able part of the phlogistic effluvia into their own  
 “ nourishment, arrest it in its progress to corrupt the sur-  
 “ rounding atmosphere. So wonderfully is every part  
 “ of

“ of the system of nature formed, that good never fails  
 “ to arise out of all the evils; to which, in consequence  
 “ of general laws, most beneficial to the whole, it is  
 “ necessarily subject. It is hardly possible for a person  
 “ of a speculative turn not to perceive, and admire, this  
 “ most wonderful and excellent provision.”

2. Since charcoal is resolvable by means of water into fixed and inflammable air, and fixed air consists of dephlogisticated air and phlogiston, these principles have been united in the ingenious experiments of Mr. Tenant, diversified by Dr. Pearson, so as to form charcoal. It was accomplished by heating substances containing fixed air, as marble &c. together with phosphorus, which contains phlogiston. This experiment has been alleged in favour of the decomposition of water; but I only see in it the composition of a substance from the elements of which it was known to consist.

3. The production of inflammable air from liver of sulphur with water, Mr. Gingembre says, arises from a decomposition of this water; because without the water no inflammable air is procured. But water, I find, is necessary to the constitution of all kinds of air, and of inflammable air most evidently.

4. Mrs. Fulhame imagines that she has proved the decomposition of water from a number of exceedingly curious experiments on the revival of metals by means of inflammable air, phosphorus, sulphur, charcoal, and various other substances of a similar nature, because the effect is never produced without the presence of moisture. Her experiments are such as I should not have expected *a priori*; and when she was so obliging as to  
 shew

shew me the result of some of them in London, I was greatly struck with them ; but I do not think that they prove the decomposition of water.

She does not pretend to exhibit separately either of the parts of which the water is said to be composed ; since she does not produce either inflammable or dephlogisticated air from this water ; and she supposes the very same quantity of water to be recomposed that is decomposed in the process. Nor does she pretend to be able to revive any metals without some substance into which phlogiston has always been supposed to enter.

All, therefore, that can be fairly inferred from her ingenious experiments is, that water assists the separation of oxygen from the calces of metals, and the entrance of phlogiston into them ; which it may do without any decomposition. Alcohol, she observes, will not answer the same purpose. But to this it is sufficient to say that alcohol is not water, and therefore has not the same properties. The presence of water is necessary to the rusting of iron, and also to the acquisition of fixed air by lime ; but the manner in which it contributes to these and other processes has not yet been ascertained.

Had she made her experiments with inflammable air in close vessels, she would, I doubt not, have found a diminution of the quantity of it, which could not be accounted for but on the supposition of its having entered into the calx, and thereby contributed to the revival of the metals.

5. She says p. 163, that “ the formation of nitrous acid in Mr. Cavendish’s noted experiment, cannot be explained on any other principle than the decomposition of water.” But Mr. Cavendish himself draws no such inference from it. All that I see in it is that phlogisticated air contains the same principle with inflammable air, viz. phlogiston; and therefore that when they are decomposed together with dephlogisticated air, they form the same thing, viz. nitrous acid. The *water* that is produced, and which adheres to the acid, I suppose to be that which is essential to the constitution of all kinds of air, and to be the greatest part of their weight.

6. Rain, she supposes, p. 167, to be water formed at the time from its proper elements in the upper regions of the atmosphere. From the respiration of fishes, and from every case of combustion, she draws the same conclusion. But in every case she says that whenever one quantity of water is decomposed, another equal quantity is composed in the same process; so that, as she says, p. 180, “ equal quantities are formed and rise regenerated like the phoenix from her ashes.” But this appears to me to be as fanciful, and fabulous, as the story of the phoenix itself.

7. From the wonderful experiments that are classed under the head of *Galvanism*, it was at first universally inferred that water was decomposed in some of the processes, because both dephlogisticated and inflammable air were produced in the water that was employed. But it was soon observed that the dephlogisticated air was no other than that which was drawn from the incumbent atmosphere; for if the communication with it



was cut off, the process stopped. Nor were the two kinds of air produced in the proportions that the new theory required in the composition of it; the dephlogisticated air being generally too little for the purpose.

From my own experiments on the subject, published in *Mr. Nicholson's Journal*, it is evident that the inflammable air must come from the decomposition of the zinc, which is calcined in the process. For when the whole pile was covered with a receiver standing in water, about one fourth of the inclosed air disappeared, and the remainder was wholly phlogisticated, exactly as it would have been if a paste made of iron filings and sulphur had been put in the place of the pile. Had any dephlogisticated air been produced, this could not have been the result, for that would have kept the inclosed air in the same state with that of the atmosphere, and the quantity would have been increased, and not diminished. The dephlogisticated air that disappeared must have gone into calx of the zinc; but certainly there had been no decomposition of the water.

Tho' the doctrine of the decomposition of water on the principle of the new theory is fallacious, my experiments make it exceedingly probable that it is resolvable into phlogisticated air, which I suppose to consist of the union of the two principles of phlogiston and dephlogisticated air. For whenever water is converted into vapour, either by means of heat, or in the vacuities of ice by freezing, a portion of this becomes permanent air, and this is always phlogisticated. For the proof of this I must here content myself with  
referring



referring to the account of my experiments relating to the *conversion of water into air*, begun in England, and those on the *freezing of water* made in this country.

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## SECTION XII.

*Of the Constitution of Fixed Air.*

IF I have proved that inflammable air comes from the *metals*, and not from the *water* in which the solution of them is made, and that water has not been decomposed, so that it cannot be proved to consist of two kinds of air, I have done all that is necessary to establish the doctrine of *phlogiston*. There are however, two other assumptions in the new theory which I think have by no means been proved, viz. that fixed air consists of *carbone* dissolved in dephlogisticated air, and that phlogisticated air, called *azote*, is a simple substance, and no compound. Neither of these suppositions appear to me to have been proved, and I think there is much positive evidence against them.

Tho' the new theory discards phlogiston, and in this respect is more simple than the old, it admits another new principle, to which its advocates give the name of *carbone*, which they define to be the same thing with charcoal, free from earth, salts, and all other extraneous substances ; and whereas we say that fixed air consists of inflammable air and dephlogisticated air, or oxygen, they say that it consists of this carbone dissolved in dephlogisticated air. See *Examination of Mr. Kirwan*, p. 79. Mr. Lavoisier says, *ib.* p. 63, that "wherever fixed air has been obtained there is charcoal." They therefore call it the *carbonic acid*.

But in many of my experiments large quantities of fixed air have been procured where neither charcoal, nor any thing containing charcoal, was concerned, or none in quantity sufficient to account for it.

There is no metal that I have ever heated with a burning lens over lime water in atmospherical air without producing a thick scum on its surface, which was, no doubt, *lime*, formed by the quicklime in the water, and the dephlogisticated air contained in the portion of atmospherical air in which the process was made. For this purpose I have tried not only iron and zinc which are said to contain plumbago (a kind of carbone from which some fixed air may be expelled) and also lead, tin, bismuth, copper, &c. as observed before, but even gold, silver, and platina, and it will hardly be pretended that all these metals contain carbone.

From a quantity of calx of lead, part grey and part yellow, in a glass tube, I got its bulk of almost pure fixed  
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ed air; and the residuum extinguished a candle. Where could be the carbone in this case?

Fixed air is always produced when iron is melted, and thereby converted into finery cinder, in atmospheric or dephlogisticated air, and also when some kinds of inflammable or dephlogisticated air are fired together. But Mr. Berthollet, Mr. Adet, and all my opponents, say that this fixed air comes from the plumbago contained in the iron, and that when it is found after the union of inflammable and dephlogisticated air, in an explosion of them, it was from plumbago contained in the inflammable air. But besides that there is no evidence of inflammable air containing any plumbago (since when iron is dissolved in any acid the plumbago is left behind) the fixed air contained in this substance, of which the antiphlogistians make so much use, is very inconsiderable; the air into which it may be resolved being chiefly inflammable.

From 6 dwts. of the purest plumbago, procured from an iron furnace, in the form of a shining black powder, I expelled, in a glazed earthen tube, forty ounce measures of air, one twelfth part of which was fixed air, and the rest inflammable, burning with a blue flame. Then, sending steam through the tube, I got two hundred and forty ounce measures more, the whole of which was inflammable air, of the purest kind, exactly resembling that from iron by the vitriolic acid. The plumbago was converted into one mass, resembling a hard cinder, and weighed two dwts and an half.

Another experiment on plumbago I shall just mention in this place. Melting one dwt. of it with a burning

ing lens in the open air, it threw off sparks, like cast iron treated in the same manner, but not quite so much; after which it was reduced to a slag like finery cinder, weighing four grains less than it had done. I repeated the experiment with the same result.

If plumbago be held in solution in inflammable air, not only must both the kinds of air contained in it, viz. fixed and inflammable, but the *slag* too, which remains after all air is expelled from it. But after the explosion which it is said discovers the fixed air that was contained in it, there is no apparent addition made to the inflammable air, nor the least appearance of the *slag*. It is evident therefore, that no such substance was contained in the inflammable air from any kind of iron, and least of all from malleable iron.

If the inflammable air had held in solution not the plumbago itself, but only the carbone that was in it, the residuum could not be plumbago, since it would want the carbone; and the inflammable air holding the carbone in solution would of the heavy, and not of the lighter and purer, kind.

Fixed air is also produced when minium, and several other substances that contain dephlogisticated air, are heated in inflammable air. This produce I had when I used some precipitate per se with which Mr. Berthollet supplied me. On being informed of this, he said that he found afterwards, that the precipitate he had sent me contained fixed air; and yet he allowed that when the air expelled from it by heat was received in lime water, it did not *immediately* make it turbid, which it is well known that a hundredth part of the fixed

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ed air that I procured by means of it would have done instantly. The turbulency that came on afterwards must, therefore, have had some other cause, probably some acid of vitriol in the water in which he made the experiment, and which gradually insinuating itself into the lime water in his tube, would form *selenite*; a thing that has frequently occurred in the course of my own experiments, and which for some time puzzled me not a little.

It is generally thought that the fixed air contained in fallen lime has been attracted from the atmosphere, in which it is said to float in a loose uncombined state. But from no other experiment that I am acquainted with can it be proved, that any fixed air necessarily exists in the atmospheric, and lime, or lime water, will become saturated with it in all situations. I am therefore inclined to think that this fixed air is composed of phlogiston imparted to the lime from the fire to which it had been exposed, and the dephlogisticated air in the atmosphere; and I have always found that a portion of atmospherical air exposed, some time to lime, or to lime water, is sensibly less pure than before; some part of the dephlogisticated air of which it is composed having been taken from it by the lime; and it is never found again except as a component part of the fixed air, which is afterwards expelled from it. The result of the experiment was the same, whether the lime was confined by water or by mercury.

The fixed air which is expelled from the yellow calx of lead which has been some time exposed to the atmosphere has, I doubt not, the same source. For when it



is heated presently after it is made, little or no air can be expelled from it, as it may some time afterwards. And I find that this substance also exposed to a portion of atmospherical air makes it less pure than it was before, just as in the case of quick lime.

As pyrophorus imbibes pure air when it is exposed to atmospherical air, leaving nothing but phlogisticated air (in which it resembles a mixture of iron filings and sulphur, which also makes a pyrophorus) the fixed air expelled from it afterwards must have been formed by the union of the dephlogisticated air imbibed by it, and the phlogiston contained in itself.

From a quantity of old and spoiled pyrophorus I got one hundred and eighty ounce measures of air, of the first part of which one half was fixed air, and the rest phlogisticated. At the last, tho' one half was fixed air, the rest was inflammable. In another experiment of this kind I found seven tenths of the air fixed, and the rest inflammable.

From 15 dwts. of fallen lime I got forty five ounce measures of fixed air, and twenty five of inflammable from the gun barrel in which the experiment was made: Whether quicklime has been exposed to the atmosphere so as to become what is called *fallen lime*, or has been saturated with water, they come in time to be of the same weight, and to have the same properties; the former continually gaining weight, and the latter losing it. From 15 dwts. of lime saturated with water and then exposed to the atmosphere, I got fifty five ounce measures of fixed air.

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I had a result similar to this when I exposed some old *plaister of Paris* to heat in an earthen retort. In these circumstances 3. 25 ounces yielded two hundred ounce measures of air, of which the first part contained about one twentieth of its bulk of fixed air, and the remainder was considerably phlogisticated, viz. of the standard of 1. 5, tho' at the last of 1. 35.

It may be said that pyrophorus attracts *water* from the atmosphere, and that the water is decomposed by exposure to heat. But in no other case is water so attracted decomposed by mere exposure to heat. Water is attracted by lime, by vitriolic acid, and various other substances; but heat will always expel it again, and it may be collected in the form of water, without any decomposition. There is, therefore, every reason to conclude that it is the same with water attracted by pyrophorus.

It is, indeed, an obvious objection to the antiphlogistic theory, that it supposes water to be decomposed in such different circumstances. What can be more so than when it is applied in the form of steam to iron red hot, also when it is quite cold, and merely present in the same vessel in which the iron, also cold, is dissolved by an acid, and by the action of light on growing vegetables, &c. &c. But if inflammable air be procured, the theory absolutely requires that water be decomposed, and no difference of circumstances is so much as attended to.

To these experiments relating to fixed air I shall add one that I made on the heating of the diamond in atmospherical air, much of the produce being fixed air.

That

That the diamond is a combustible substance has been long known, but not having heard of its being burned in atmospherical, or any kind of confined air, I long wished to do it; and being supplied by a friend with two small ones, I treated them in this manner, and found that near ninety parts in one hundred of the air in which they were burned was completely phlogisticated, and the quantity not being sensibly changed, the remainder was fixed air; which is an effect similar to that of heating charcoal of copper in air. The diamonds being very small, and the quantity of air in which they were burned being very small in proportion, I will not vouch for much exactness in the result, tho' the experiment was carefully made over mercury. Both the diamonds weighed only three tenths of a grain, and they lost in the process fifteen hundredths of a grain. The air in which they were burned was three fourths of an ounce measure.

With diamonds furnished me afterwards by Mr. Jeffries, a jeweller in London, I made the following experiments.

In 4.5 ounce measures of dephlogisticated air (of the standard of 0.6, with two equal quantities of nitrous air) I heated a diamond that weighed one seventh of a grain till it intirely vanished, after which the air was reduced by means of lime water to an ounce measure and an half of the standard of 1.92.

The weight of the fixed air produced greatly exceeding that of the diamond, it is evident that the substance of it must have united with the dephlogisticated  
air

air to form the fixed air. As the weight of this air was nearly three grains, and that of the diamond about a fourth part of it, the proportion of dephlogisticated air in fixed air must be the same that I found it to be in my former experiments.

According to this experiment, the *diamond* and *charcoal of copper*, tho' so different in appearance (the one solid and beautifully transparent, and the other a black and friable substance) are both as nearly as possible pure phlogiston. For tho' something, no doubt, remains after their dispersion, it is too small to be discovered.

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### SECTION XIII.

#### *Of Phlogisticated Air.*

**T**HE subject of phlogisticated air is attended with considerable difficulty. I am able, however, to produce sufficient evidence that it is not, as the antiphlogistians say, an elementary substance, but that it derives its origin from phlogiston; since it may be generated

generated from inflammable air, sometimes alone,\* but in other cases by means of dephlogisticated air, whether any portion of this kind of air be united with it or not. Of this my readers will judge from the experiments that I shall lay before them.

1. One decisive proof that phlogisticated air may be formed, and seemingly by the union of dephlogisticated air and phlogiston, is the quantity of phlogisticated air that remains after any explosion of dephlogisticated or common air with inflammable air, more than what remains after the mixture of nitrous air with either of them.

Having procured a quantity of dephlogisticated air so pure that one measure of it mixed with two measures of nitrous air was reduced to 0. 04, I could not by any mixture of the purest inflammable air fired along with it reduce it to less than 0. 25.

2. The very different proportions in which atmospheric air is diminished in different processes, is a proof that in some of them there must be a generation of phlogisticated air. When air is diminished by iron filings and sulphur moistened with water, the proportion of phlogisticated air is that which Mr. Lavoisier states, viz. 73 parts in 100. But when I made the mixture without any water, I found that one hundred measures were in six days reduced only to ninety completely phlogisticated, which is in the proportion of 81. 8, in 100. Again one hundred and forty ounce measures

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\* I am now pretty well satisfied that when inflammable air becomes phlogisticated air, it gets dephlogisticated air through the water by which it is confined.

were by the same dry mixture reduced to one hundred and thirteen, which is in the proportion of 80. 6, in 100. But some water getting to the mixture the third time that it was used, one hundred and fifty five ounce measures of air were reduced by it to one hundred and sixteen ; which is in the proportion of 74 to 100.

By the slow burning of phosphorus sixty ounce measures of atmospherical air was reduced to forty eight ; at another time to 48. 5 ; and fifty ounce measures were reduced to forty, which is in the proportion of 80 parts of phlogisticated air in 100. But by repeatedly firing the phosphorus with a burning lens, one hundred ounce measures were reduced to eighty nine completely phlogisticated.

Messrs. Berthollet and Fourcroy however, say (*Annales de Chymie*, Vol. 26, p. 308) “ We must abandon  
“ the test of the purity of air by means of nitrous air ;  
“ and substitute that of phosphorus, by means of which  
“ we get uniform results. They are different with ni-  
“ trous air, on account of the different proportions in  
“ which this air combines with dephlogisticated air to  
“ form nitrous acid.”

But how can these proportions vary when the circumstances in which they are mixed are exactly the same ? The nitrous air admitted in the same manner to any kind of air, containing in it a portion of dephlogisticated air, must unite with this, and this only ; leaving the other, with which it cannot combine, as it was ; and it requires no particular degree of heat to do this. The result is, therefore, the same in all temperatures. On this account it is the most convenient, and perhaps



as accurate test as we can apply. It is only necessary that there be a sufficient quantity of nitrous air to saturate all the dephlogisticated air that it can meet with, and that the same time intervene between the mixture and the measuring of the diminution occasioned by it. The diminution of atmospherical air by means of phosphorus is both a tedious, and even a less certain process, as well as attended with expence ; and I find that the use of inflammable air instead of nitrous air, which some persons recommend, is liable to much greater objections, the result of the firing of them by the electric spark being exceedingly various in circumstances as nearly as we can judge the very same.\*

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\* Mr. Rupp also objects to the use of nitrous air as a test of the purity of atmospherical air, and quotes a former experiment of mine, in which it appeared that by only pouring a mixture of nitrous and atmospherical air from one vessel to another, and also by letting the mixture stand some days without any agitation, the degrees of diminution were very various ; and he says that therefore from the still greater diminution of this mixture which I have since observed, it cannot be concluded that atmospherical air contains more dephlogisticated air than has hitherto been supposed. I acknowledge that my conclusion from that observation was not just, but for a reason that I was not at that time acquainted with. For I have since found that not only does that mixture of air continue to diminish still farther by being longer confined by water, but that a quantity of any kind of air will in time be wholly absorbed in the same circumstances, and that some time before they disappear they all become phlogisticated air, inflammable air as well as the rest.

This is a fact of which I am not able to give any rational account, any more than of several others that have fallen under my observation. I have given a detailed account of the facts in an article I lately sent to the philosophical society at Philadelphia, together with another, on any two kinds of air, separated by an earthen vessel, or a bladder, changing



3. Since pure nitrous air wholly vanishes when it unites with pure dephlogisticated air, the phlogisticated air that is found after heating iron in it cannot be a simple element, but must have been formed from something in the nitrous air and phlogiston from the iron. Heating malleable iron in sixty ounce measures of nitrous air, it was reduced to twenty four, all phlogisticated. When I continued this process beyond the point of greatest diminution, the air produced was inflammable.

4. Since water contains but a small quantity of air in proportion to its bulk, and generally considerably purer than that of the atmosphere, the phlogisticated air that is produced by heating steam in a copper vessel must have been formed from phlogiston in the copper and the air contained in the water; and whenever I have heated water in this manner (the upper part of a closed copper tube being kept in a red heat, while the lower and open part was immersed in water) I have found a considerable quantity of air completely phlogisticated, and the longer I kept it in this state the more of this air I found. I had similar results when I used a silver tube.

That this phlogisticated air is not that which had passed from the centre of the fire thro' the metal tubes (tho' some of my late experiments prove that some metals

places, which I had observed before with respect to steam and air. This is a fact of great importance in the system of nature, especially with respect to respiration; but of the cause of it I have not even a conjecture worth proposing. Both the above mentioned articles are published together with others, in the fifth volume of the Transactions.

ta's are permeable to air in these circumstances) appears from the results of my experiments with glazed earthen tubes in the same circumstances. For the air that gets into the inside of these is often little worse than atmospherical air.

5. It is well known that hot charcoal imbibes any kind of air ; and I have observed that when it is afterwards put into water it gives this air out again. But if the air be that of the atmosphere, it takes the dephlogisticated part in preference to the other, leaving the remainder phlogisticated ; and the air that it gives out after this in water is chiefly phlogisticated. What, then, becomes of the deplogisticated air that has disappeared ? Will it be said that it remains in the charcoal, which had imbibed it ? Whence then came the phlogisticated air which it gave out, when, according to the new theory, charcoal does not contain any such principle ? It is not found in the water into which it is put ; for this gives out air less pure than it did before the process.

6. A solution of copper in volatile alkali gave phlogisticated air with marine acid, and it will not be easy to say where this azote existed before the process.

7. Most of the substances which have been used to phlogistificate air gain an addition to their weight in the process, in consequence of which it has been taken for granted by the antiphlogistians, that nothing is emitted from them, and that they only imbibe the dephlogisticated air, which is one constituent part of the atmosphere, leaving the other part, which they call *azote*, unaltered. It was, therefore, desirable to find  
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some substance which would not gain any weight in the process, and yet have the same effect in phlogisticating the air. For the dephlogisticated air not uniting with the substance exposed to it must necessarily form some other combination.

This end was in some measure answered by *steel*, which, according to the common hypothesis, containing more phlogiston than iron, would, I thought, part with more on the application of heat and receive less addition; and this I found to be the case. But it was more completely answered by *black bones*, which without gaining any thing by the application of heat in any circumstances, became white in the process.

If this be done in common air, as the bones do not imbibe the dephlogisticated air that disappears, this air is disposed of in two different ways. For one part of it contributes to form fixed air, and another part may form a different union with something emitted from the bones, and make an addition to the phlogisticated air. Accordingly, there is more of it found after the process with the black bones than with iron, and many other substances which receive an addition of weight in the process.

Whence, then, I ask, can come this addition of phlogisticated air, but from an union of phlogiston committed from the bones, with the dephlogisticated air in the atmospherical air to which they are exposed? Consequently phlogisticated air, or azote, is not a simple substance, as the antiphlogistians maintain, but a compound. Also whence can come the fixed air that is procured in the same process, but from a different com-

bination of the same elements, and not as they say from *carbone*, which is a substance of vegetable origin, and has no place here? Mr. Rupp is of opinion that the fixed air is formed from the *carbone* in the bone, and the dephlogistiated air that disappears. But when the heat is applied with care, there is no loss of weight in the bone; so that nothing is driven from it besides the *phlogiston*, which appears to have no weight at all, or none that we can ascertain.

That the thing which constitutes the blackness of the bones is the same with that which has always been called phlogiston, is evident from its forming inflammable air if there be water to supply it with a basis. For I find that if they be heated in phlogistiated air, which cannot by parting with any thing contribute to this whiteness, they nevertheless become white; the air in which they are heated is increased in quantity, and this increase is inflammable air.

For these experiments I find *ivory black*, which is the coal of ivory used by painters, more convenient than common bones. To prepare this substance for the experiment, I fill an earthen tube with it, and closing it with clay, expose it for a considerable time, at least a quarter of an hour, to the greatest heat of a smith's fire, which will expel from it every thing that is volatile; so that no heat to which I can expose it afterwards will affect it, except by means of some other substance with which that which constitutes its blackness has an affinity, and with which it can combine.

Heating a quantity of ivory black prepared in this manner in 6. 5 ounce measures of atmospherical air,  
there

there was no sensible change in the quantity; but on examining it, I found in it an ounce measure of fixed air, and the remainder was completely phlogisticated, which is in the proportion of 84 parts in 100; whereas the antiphlogistians say that any proportion of atmospherical air contains only 73 parts in 100 of phlogisticated air. It is evident, also, that both these substances consist of the same elements, viz. dephlogisticated air and phlogiston.

A writer in the *Medical and Physical journal*, p. 30, finds no production of phlogisticated air, but only of fixed air, by heating a black bone in dephlogisticated air, and therefore he concludes that my experiment with atmospherical air was inaccurate. But he should have used the same kind of air that I did. What I have observed is that sometimes fixed air, and sometimes phlogisticated air is produced from the same elements, tho' I have not been able to discriminate all the circumstances in which one or the other is the result of their combination.

7. Having made much use of a mixture of *iron filings and sulphur*, for the purpose of phlogisticating air, I have always had a large quantity which had been long exposed to the atmosphere, from which it is allowed that it attracts nothing besides dephlogisticated air. Of this mixture, become quite dry and brown, three and an half ounces exposed to heat in an earthen tube gave out one hundred and twenty ounce measures of air, of which about one tenth was fixed air, and the rest almost wholly phlogisticated. Both these kinds of air, therefore, must consist of dephlogisticated air from



the atmosphere, and something contained in the iron, or the sulphur, both of which are maintained to be simple substances. There remained a black powder, strongly attracted by the magnet.

8. In general iron filings and sulphur immersed in mercury or water, or placed in a vacuum, yield inflammable air; but in some cases (tho' I do not know the reason of the difference) this mixture has yielded phlogisticated air.

Having placed a pot containing some of this mixture in a vacuum, I found after some days that it had yielded two and an half ounce measures of air; and examining it, I found it to be completely phlogisticated. I then put the same mixture under water, and placing it near the fire, it gave an ounce measure more, all phlogisticated.

At another time two ounce measures of air were yielded by a mixture of this kind; and being examined, tho' not till long after it was formed, it was found to be wholly phlogisticated. It might have been originally inflammable air, and afterwards have changed to phlogisticated.

9. Of the change of inflammable air into phlogisticated air, several instances may be seen in the account of some of my early experiments; but I am not yet able to say on what this change depends. Supposing that it required the union of a portion of dephlogisticated air, I exposed to it pieces of iron, which being covered with rust, had attracted and contained that air; but the results were not uniform. I shall, therefore, content myself with relating what I observed, wishing that o-  
the



ther persons may diversify the circumstances, and endeavour to ascertain the cause of the different results.

Having made a number of pieces of iron rusty by dipping them in marine acid, I put them into a glass vessel which I then filled up with mercury, and I displaced this mercury by inflammable air. After waiting about eight months, I examined the air, and found it to be very slightly inflammable, the far greater part of it being evidently phlogisticated air. The iron, from being red, which all antiphlogistians will say was owing to its containing oxygen, was become black, being covered with a kind of foot, which was wiped off, staining the fingers and paper. Under this coating the iron was of its usual colour. Whence, now, came this phlogisticated air, if not from the union of dephlogisticated and inflammable air?

This experiment is very little liable to the objection of the Monthly Reviewer, p. 371, as the pieces of iron had not been exposed to the atmosphere any great length of time; and I am confident that by no process whatever could any phlogisticated air have been extracted from them.

If the above mentioned black substance with which the pieces of iron were coated be *plumbago* (and of this little doubt can be entertained) it will appear to be a calx of iron supersaturated with phlogiston, and that the whole of the iron might have been converted into it; but that plumbago cannot be contained in iron, so as to yield, on its solution in an acid, the phlogisticated air of which my opponents have endeavoured to avail themselves.

On the 13th of August 1799, I examined a quantity of inflammable air which had been confined by mercury with dry iron rusted in nitrous acid, from the 13th of March 1798, and found nothing inflammable in it, tho' there was no apparent change in the colour of the iron. This was also the case with another quantity of the same kind of air which had been confined in the same manner from the 14th of July. However, another quantity of inflammable air that had been confined the same time, and in the same manner, with iron rusted in vitriolic acid was not much changed, tho' the iron was become black.

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## THE CONCLUSION.

**B**EFORE the new theory of chemistry can be unexceptionably established, the following things must be done.

1. Whenever inflammable air, or *hydrogen*, is procured, evidence must be given of the production of a due proportion of *oxygen*, that is of 85 parts of this to 15 of the other ; and this evidence must be something more than an addition of *weight*. It must be either actual *acidity*, or *dephlogisticated air*. Otherwise there

is no proof of the inflammable air having come from the decomposition of the water. This, however, has not been done with respect to iron, or any other substance by means of which inflammable air is procured.

2. When water is procured by the burning of inflammable air in the dephlogisticated air, not only must the water be free from acidity, but there must have been no production of phlogisticated air in the process. For by the decomposition of this air the nitrous acid may be procured.

On the whole, I cannot help saying, that it appears to me not a little extraordinary, that a theory so new, and of such importance, overturning every thing that was thought to be the best established in chemistry, should rest on so very narrow and precarious a foundation; the experiments adduced in support of it being not only ambiguous, or explicable on either hypothesis, but exceedingly few. I think I have recited them all, and that on which the greatest stress is laid, viz. that of the formation of water from the decomposition of the two kinds of air has not been sufficiently repeated. Indeed, it requires so difficult and expensive an apparatus, and so many precautions in the use of it, that the frequent repetition of the experiment cannot be expected; and in these circumstances the practised experimenter cannot help suspecting the accuracy of the result, and consequently the certainty of the conclusion.

But I check myself. It does not become one of a minority, and especially of so small a minority, to speak or write with confidence; and tho' I have endeavour-

ed to keep my eyes open, and to be as attentive as I could to every thing that has been done in this business, I may have overlooked some circumstances which have impressed the minds of others, and their sagacity is at least equal to mine.

Tho' the title of this work expresses perfect confidence in the principles for which I contend, I shall still be ready publicly to adopt those of my opponents, if it appear to me that they are able to support them. Nay, the more satisfied I am at present with the doctrine of phlogiston, the more honourable shall I think it to give it up upon conviction of its fallacy; following the noble example of Mr. Kirwan who has acquired more honour by this conduct than he could have done by the most brilliant discoveries that he could have made.

The phlogistic theory is not without its difficulties. The chief of them is that we are not able to ascertain the *weight* of phlogiston, or indeed that of the oxygenous principle. But neither do any of us pretend to have weighed *light*, or the element of *heat*, tho' few persons doubt but that they are 'properly *substances*, capable by their addition, or abstraction, of making great changes in the properties of bodies, and of being transmitted from one substance to another.

As to the *new nomenclature*, adapted to the new theory, no objection would be made to it, if it were formed, as is pretended, upon a knowledge of the real constitution of natural substances; but we cannot adopt one, the principles of which we conceive not to be sufficiently ascertained. For other objections to this nomenclature, I refer to the preface of Mr. Keir's excellent

lent *Dictionary of Chemistry*. However, whether we approve of this new language or not, it is now so generally adopted, that we are under a necessity of learning, tho' not of using, it.

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## A P P E N D I X.

### 1. Of Dr. Mitchill's Attempt to reconcile the two Systems.

**D**R. MITCHILL, professor of chemistry in the college of New-York, to whom, as an impartial and excellent judge of the subject, I addressed my Letters in defence of the doctrine of phlogiston in the *Medical Repository*, has employed much ingenuity in an essay in that work (Vol. 1, p. 514) on a scheme calculated to reconcile the phlogistians and antiphlogistians. But his proposal will hardly be admitted by either of the contending parties.

The phlogistians will not admit that water contains an inflammable principle merely because the blast of

an eolipyle will promote the burning of fuel ; since whenever this is the case, a current of *air* always accompanies the current of *steam* ; and if this be prevented the steam extinguishes the fire as effectually as cold water, or phlogisticated air.

On the other hand the antiphlogistians will not acknowledge that even common sulphur, phosphorus, iron or zinc, contain any hydrogen, which Dr. Mitchell makes synonymous to phlogiston. And the phlogistians maintain that if any of these substances contain phlogiston, they all must, and every metal without exception, as gold, and others which he thinks contain none ; because the calces, or bases, of them all become these substances in consequence of imbibing inflammable air ; and because either this air or nitrous air (which contains the same principle) is evolved whenever they are dissolved in acids. In short, the metals, as well as sulphur and phosphorus, are either necessarily simple substances, or necessarily and universally compounds ; and water is either resolvable into two kinds of air, or it is not ; and upon the decision of these questions the whole controversy hinges.



2. *Of the Discovery of the Production of dephlogisticated Air by the Action of Light on Plants.*

**I**N this work I call the discovery of the emission of dephlogisticated air by the action of light upon plants *mine*, whereas writers in general give it to *Dr. Ingenhousz*. That justice may be done to us both, I shall copy a *Note* introduced into my *Experiments on the generation of air from water*, which was printed in the year 1793, and also a letter of his to me which I have since found among my papers (which were partly destroyed, and partly dispersed, in the riots of Birmingham) from which it will appear that we do not differ in opinion with respect to any *facts*, but only with respect to the degree of merit that belongs to each; and of this others will be more impartial judges than either of us.

“ As some persons imagine there is an interference  
 “ in Dr. Ingenhousz’s claims to discovery and mine,  
 “ I shall subjoin an extract of a letter I wrote to him  
 “ six years ago, when a young physician on his travels  
 “ desired of me letters of introduction to my philosophi-  
 “ cal friends on the continent, since it will tend to set  
 “ the matter in a proper light, and shew that there is no  
 “ ground of interference between us at all.”

DEAR

Birmingham, Nov. 21, 1787.

DEAR SIR,

" I THANK you for the French edition  
 " of your *Experiments on Vegetables*, which I received  
 " some time ago ; but I am sorry to see in the Preface  
 " something that looks as if you or your friends,  
 " thought I wished to detract from your merit, which  
 " is very far from my disposition."

" I do not, indeed, distinctly see what ground there  
 " is for any interference between us. That plants  
 " restore vitiated air I discovered in a very early peri-  
 " od. I afterwards found that the air in which they  
 " were confined was sometimes even better than atmos-  
 " pherical air, and that the *green matter* (which I at  
 " first, and several of my friends always thought to be  
 " a vegetable) produced pure air by means of light ;  
 " and immediately after the publication of this fact,  
 " and before I saw your book, I found that other  
 " whole plants did the same."\*

" All

\* Dr. Ingenhousz, however, says in his *Essay on the food of plants*,  
 printed in the *Additional Appendix to the proposed General report from the  
 board of Agriculture*, p. 14, that I " absolutely deny all emission of air  
 " from the surface of plants as well as from the skins of animals." The  
 latter I certainly do deny ; and I think I have given sufficient evidence  
 of the truth of my opinion. As to plants, I observed that not only the  
 leaves, but the green stalks, gave dephlogisticated air when the sun  
 shone upon. That they give *any* air in the dark, when they are in a  
 healthy state, tho' maintained by Dr. Ingenhousz, I never found.  
 According to him, the injury they do to the atmosphere in the night  
 tends to counteract the service they render to it in the day.

“ All the time that I was employed in making these  
“ experiments I wrote to my friends about them, par-  
“ ticularly to Mr Magellan, and desired him to com-  
“ municate my observations to you, as well as to others ;  
“ but I believe you did not hear of them, so that what  
“ you did with *leaves* was altogether independent of  
“ what I was doing with *whole plants* ; but the same  
“ summer, and the same sun operated for us both, and  
“ you certainly published before me.”

“ This appears to me to be the true state of the  
“ case ; and surely it leaves no room for the suspicion of  
“ any thing unfair, or unfriendly. But whatever your  
“ friends may say, I have no thought of troubling the  
“ Public with any vindication. I value you, and your  
“ friendship, too much to wish to have any altercation  
“ on the subject. Indeed, there is nothing to contend  
“ about. If on any future occasion you will do me the  
“ justice to give this statement of the matter, I shall be  
“ happy. If not, I shall not complain—I shall always  
“ be happy to hear from you, and am

Dear Sir,

Yours sincerely

J. PRIESTLEY.”

" This I think will shew that I was not very anx-  
 " ious about the merit of this discovery. The original  
 " observation that plants restore vitiated air, and the  
 " subsequent one, that *green vegetable matter* yields de-  
 " phlogisticated air by means of light, were both acci-  
 " dental ; and surely there was no great sagacity re-  
 " quired to try the effect of *certain plants*, when I had  
 " ascertained the fact with respect to one of a *doubt-*  
 " *ful* nature. That *leaves* had this power, I own I had  
 " no suspicion. But the merit of all philosophical  
 " discoveries is, in my opinion, greatly overated."

The following is the letter I received from him  
 some time after.

London, July 7, 1790.

DEAR SIR,

" RECEIVE my hearty thanks for the va-  
 " luable present of your philosophical work in three vo-  
 " lumes."

" As I do not know whether I understood rightly  
 " the seventh section of the third volume, I was at the  
 " first reading of it somewhat puzzled, and thought  
 " that the contents of it might induce the reader to be-  
 " lieve that *you*, and not *I*, were the first who published  
 " that it is *the light of the sun that is the cause why real*  
 " *plants correct bad air, and yield vital air*, tho' all those  
 " of our common friends whom I consulted on this  
 " head are of opinion that such an assertion is by no  
 " means intended by you. If you have really published  
 " this

“ this doctrine before me, I owe you the justice to acknowledge it publicly in the first volume of my books that will be published, or reprinted ; and I will cheerfully retract, by acknowledging that in reading your philosophical works I have through inadvertence, and not design, overlooked this doctrine ; and I will very readily quote the volume of your works, and the page in which you will inform me this doctrine is clearly and explicitly to be found. But if this doctrine was first published by me, as I have till now been persuaded is the case, I will leave things as they are. The confidence I have in your liberal manner of thinking makes me hope that you will favour me soon with an answer, which will be very gratefully received by

Your obliged

humble servant,

J. INGENIOUSZ.”

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THE copy of my answer was destroyed in the riots ; but after stating to him the above mentioned circumstances (which if he had attended to in the preceding letter, would have saved him the trouble of writing this) and the degree of merit to which I thought him

him entitled in the business, I concluded with saying, that for his sake I wished it had been greater than it was.

It is obvious to remark that if the merit of this discovery, be it more or less, depend, as he here acknowledges, on the substance which yields dephlogisticated or vital air by means of light being a *real plant*, all philosophers except Dr. Ingenhoufz himself, will, I doubt not, give it to me; because the *green matter* which I have found to give this air by means of light, is acknowledged to be a vegetable.

Dr. Ingenhoufz, however, maintains a very singular opinion on the subject. For he says p. 90, that “the water itself, or some substance in the water, is, I think, changed into this vegetation.” This is nothing less than reverting to the long exploded doctrine of *equivocal generation*. For if this vegetable could have its origin from mere water, or any thing in the water that was not a vegetable, or the seed of one, an oak might arise from water, or something in water that was not an acorn.

“This real transmutation,” he however says, “tho’ wonderful in the eye of a philosopher, is no more extraordinary than the change of grass and other vegetables into fat in the body of a graminivorous animal, and the production of oil from the watery juice of an olive.” Other philosophers will, I doubt not, see these cases to be very different; and why Dr. Ingenhoufz alone should hesitate to call this *green vegetable matter* (as he himself terms it) to be a *proper plant*, with the power of propagating itself, and of producing dephlogis-



dephlogisticated air like other plants, cannot, I think, be accounted for, but from his persuasion, that if it be a real plant, arising from a seed, like other plants, there would remain little that he could claim in the merit of this discovery.\*

When I had made, and published, this discovery, which was before he had made any experiment on the subject, and only hesitated to call it a vegetable because, when I examined it with a microscope with that view, I could not discover the form of one, could I forbear examining the first opportunity whether other *certain plants* would do the same; and would not a generous friend have forborne to anticipate me in a discovery, which if not already made, I was known to be in pursuit of, and could not possibly miss. Confiding in this, tho' I completed the discovery the next summer, at the same time that Dr. Ingenhousz made his experiments, I did not make the haste that he did in publishing it; but, contenting myself with announcing it to my friends, deferred the publication till I had materials for another volume. When I, according to my known custom, was writing to acquaint my friends with what I had done, Mr. Magellan, I well remember, said that Dr. Ingenhousz kept himself very close, and no body knew what he was about till his book was printed.

If, however, the *making a thing known* by speaking or writing without printing, be *publishing*, my publication

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of

\* In his *Essay on the food of plants*, he represents it as my opinion that this *green matter* is a substance *sui generis*, and consequently not a proper plant, tho' I used that language only till I had satisfied myself that it was one, and afterwards never intimated a doubt on the subject

of plants in general yielding dephlogisticated air by means of light, as well as this property of the green vegetable matter in particular, was prior to that of Dr. Ingenhoufz. It is well known to all my friends and acquaintance that I never deferred this method of publishing my discoveries a single day, if I had an opportunity of communicating them to others by speaking or writing about them. Besides that this was always my natural inclination and habit, I have more than once observed that the speedy communication of discoveries is of great importance to the advancement of science.

The several steps in this investigation were as follows. In 1772, I found that the growth of plants restored air vitiated by animal respiration. For this discovery chiefly I received the gold medal of the Royal Society; and Sir John Pringle, in his speech on the occasion, enlarged on my idea of one part of the creation being the means of repairing the injury done to the atmosphere by the other. In 1778, being at Limington, on the sea shore, I found the air in the bladders of the sea weed to be much purer than that of the atmosphere. In the same summer I found the air in which some plants had grown much purer than the external air, an effect that could not be ascribed to any thing but the production of dephlogisticated air. And it was at the close of the same year that, observing bubbles of air emitted by the green matter with which the inside of some of my phials was covered, I examined it, and found it to be highly dephlogisticated. Excluding the *light*, the production of air always ceased, tho' in the same degree of *heat*; so that the effect was owing to light only.

Being

Being in London the winter following, I shewed this experiment to all my friends, and among the rest to Dr. Ingenhoufz, who was particularly struck with it. The question amongst us then was what this *green matter* could be; and it being generally thought to be a *vegetable*, I determined to try the effect of known plants as soon as I should return to the country. Accordingly I did so with the first sun shine that I had, and completed the discovery. But in the mean time Dr. Ingenhoufz anticipated me by his publication, which I think I should not have done with respect to him, if I had found him in the same train of investigation in which he found me.

Such are the *facts*. Let others judge of them as they see reason. The ascertaining any person's right to the discoveries he makes is of no farther use than as a motive to others; shewing them that they will not lose the share of praise to which their sagacity or industry shall fairly entitle them. As it is now more than twenty years since the discovery was made, in all which time Dr. Ingenhoufz has enjoyed the merit of it, I cannot be said to have shewn much anxiety about it.

Dr. Ingenhoufz in his *Essay on the food of plants*, p. 2, speaks of my "known eagerness for fame," and also that of Mr. Scheele. It has not, however, been very conspicuous in this business; and if it be a fault, I think Dr. Ingenhoufz himself will be thought to be as chargeable with it as either of us:

3. *Of the Discovery of dephlogisticated Air.*

NOW that I am on the subject of the *right to discoveries*, I will, as the Spaniards say, leave no ink of this kind in my inkhorn ; hoping it will be the last time that I shall have any occasion to trouble the public about it.

Mr. Lavoisier says (*Elements of Chemistry*, English translation, p. 36) “ this species of air ” (meaning dephlogisticated) “ was discovered almost at the same time by Mr. Priestley, Mr. Scheele, and myself.” The case was this. Having made the discovery some time before I was in Paris in 1774, I mentioned it at the table of Mr. Lavoisier, when most of the philosophical people in the city were present ; saying that it was a kind of air in which a candle burned much better than in common air, but I had not then given it any name. At this all the company, and Mr. and Madame Lavoisier as much as any, expressed great surprise. I told them that I had gotten it from *precipitate per se*, and also from *red lead*. Speaking French very imperfectly, and being little acquainted with the terms of chemistry, I said *plomb rouge*, which was not understood till Mr. Macquer said I must mean *minium*. Mr. Scheele’s discovery was certainly independent of mine, tho’ I believe not made quite so early.

## 4. Of Mr. Humphry Davy's Essays.

WHEN some progress was made in printing this work I met with *Dr. Beddoes's Contributions to physical and medical knowledge*, and in it Mr. H. Davy's *Essays*, which have impressed me with a high opinion of his philosophical acumen. His ideas were to me new, and very striking, but they are of too great consequence to be decided upon hastily. I wish so seemingly accurate an experimenter would repeat, with the attention that he would give to all the circumstances, the French experiment of the composition of water. I cannot help suspecting that his account of it would not be so very decisively in favour of their conclusion as theirs.

Mr. Davy takes it for granted that water is decomposed by the growth of plants, and thinks this to be proved by finding dephlogisticated air produced in this manner in water out of which air had been expelled by boiling, or by the air pump. But he was not aware that water even recently boiled, and examined while warm, contains nearly as much air as it did before boiling, and by no means so pure; so that it can probably supply more nourishment to a plant than water which had not been boiled. Air expelled from water by the air pump, or even the Torricellian vacuum, which does it more effectually, is soon replaced by exposure to the atmosphere.

What



What I complain of in Mr. Davy and many others, is the too hasty introduction of *new terms* in chemistry. They perplex those who are most conversant with the subject, and are a great impediment to beginners. For the old language will never be wholly obsolete while old books are read. If the new theory should not stand its ground, many terms in the new nomenclature must fall with it. And Mr. Davy's *nitrogen*, I suspect, will be no longer lived than the French *hydrogen*.

I have myself been exceedingly cautious in introducing new terms, and have never done it but when there was an absolute necessity to give a name to a substance that had no name before. Air rendered unfit for respiration or combustion, having no name appropriated to it. I, having frequent occasion to mention it, called it *phlogisticated* air, because atmospherical air I found was reduced to that state by substances containing phlogiston, if there be such a thing as phlogiston (which was then universally taken for granted) and by no other means. Having afterwards discovered another kind of air, the properties of which were the reverse of those of phlogisticated air, I called it *dephlogisticated* air. I also introduced the terms *nitrous* air, *dephlogisticated nitrous* air, *Marine acid* air, *vitriolic acid* air, *fluor acid* air, *alkaline* air, and *sulfurated inflammable* air, denominating them from their supposed constituent parts, or their known properties, and I generally consulted some philosophical friend before I fixed upon any of them.

I saw no occasion for the term *gas*, because I found the term *air* used generically already; the term *fixed* air and *inflammable* air, as well as *atmospherical* air be-

ing



ing in use before: If we want an adjective, the *aerial form* of a substance will do as well as the *gazeous form*; and Mr. Davy, who introduces the term *gazity* to express the abstract idea in the substantive form, might have made as good a word of a similar signification from *air*.

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